THE PSYCHOLOGY OF HUMAN LEARNING

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INTRODUCTION TO THE FIRST EDITION

DURING the past thirty years there has been an amazing development in the field of human and animal learning. The literature is now so extensive that its mastery by any one person is an almost insuperable task. There have been accumulated a wealth of empirical data and generalization, of special and general theory, and a variety of points of view and of interpretative systems. Naturally, theories and interpretations exhibit much disagreement. Many generalizations may be regarded as valid for the present, others must be regarded as tentative and questionable because of the lack of reliable or adequate data, while with some topics no generalizations are permissible because the empirical data are in direct conflict.

Throughout his professional life this field has been the author's prime research and teaching interest, and he soon came to feel the need for a comprehensive reference manual which would attempt to organize this diverse material in a systematic fashion, present the pros and cons for the various theories and points of view, make such generalizations as are justified for the present, note the unreliable, inadequate, and apparently conflicting data, and point out the type of research needed to resolve these conflicts and make possible valid generalizations. It was assumed that such a comprehensive manual would prove to be of service to advanced students and investigators, and would exercise a stimulating and directive influence upon the course of subsequent investigation.

Some years ago the author proposed to write a two-volume manual with the advice and critical help of the writer. The author's first task was to read and digest every worth-while article of this extensive literature, in the course of which he accumulated a mass of typewritten digests and analyses. He then undertook to write the first draft of the manuscript without much regard to limitations of space. As these chapters were completed, they were turned over

to the writer, who added to each chapter an extensive series of critical and constructive comments and interpretative analyses, and it was in the light of these that the author proposed to condense and revise this first draft into the final manuscript.

In the autumn of 1936 this first draft was about four-fifths completed. Because of the demands of health, a change of location, and the assumption of new teaching and administrative duties, the completion of the work was postponed for several years. When he was ready to return to this work in 1940, he was persuaded to write first the present textbook for students. He consented to do this partly because he felt that there was a need for such a text, but primarily because he felt that the necessary condensation and revision involved would prove invaluable in writing the final draft of the larger work in which he was primarily interested.

Not having seen the manuscript, the writer cannot pass judgment upon its merit as a text nor can he comment upon its distinctive features. However, a text of this sort must necessarily be selective with respect to the topics treated and their relative emphasis. It must also be written in a more positive and dogmatic tone—much more expressive of the author's views and evaluations than would have been the case with the projected manual.

The author was thoroughly qualified to write such a text. It was written in the prime of life with a background of considerable research and teaching in this, his favorite field. He had an extremely able and vigorous mind. He was independent in judgment, intellectually honest, and thoroughly imbued with the ideals of scientific scholarship. He was a prodigious worker and had a first-hand knowledge of every worth-while article in the literature. He had contributed chapters on Learning to some of our published texts and, finally, he had the experience in writing the first draft for the projected manual. Whatever merits this text may finally be judged to possess, there is no doubt that it will be regarded as a thoroughly sound and scholarly piece of work.

The manuscript was completed and sent to the publisher shortly before his untimely death. Unfortunately, no preface had been written, and this brief introduction must serve in its stead. If such a

preface had been written, the author would undoubtedly have expressed his thanks and appreciation to many who gave him aid and encouragement: among them, to A. W. Melton, H. N. Peters, and K. W. Spence for their critical reading of the manuscript; to the stimulating group of graduate students with whom he discussed the chapters; to J. B. Stroud, who compiled the subject index and settled puzzling points in the proofreading; and to Mildred Atwood, his secretary, for her untiring labors at typing the manuscript, reading proof, and otherwise preparing the book for publication.

His thanks are due the many authors who have so willingly permitted him to use materials from their works.

Harvey A. Carr

INTRODUCTION TO THE SECOND EDITION

THE psychology of human learning has undergone great changes since this book appeared in its first edition in 1942. Not only has an enormous quantity of factual knowledge been accumulated during this time, but also certain shifts of emphasis and orientation have occurred. There is, today, a far greater *rapprochement* between the studies of human and animal learning than was true a scant decade ago. Emphasis upon theoretical aspects of the learning process has become much greater, and, indeed, the term "learning theory" has become changed in its meaning during this time. At the same time, there has been a significant increase in the degree to which facts and theories of learning have been applied to the solution of practical and theoretical problems in other areas. It is difficult to view these developments in a proper perspective. They have occurred so recently and are so involved with our present interests and enthusiasms that it is difficult not to feel that they are more significant than history will prove them to be, and while we may be aware that the events of the past decade have quite revolutionized the field of learning, yet we should be prepared to find, ten years hence, that this revolution has not been entirely fruitful.

The changes in the field of learning, no matter how we may regard them, have been reflected in the revision of this volume. The reader who is acquainted with the earlier edition will note that the organization of the book has been changed somewhat, that chapters dealing with theoretical problems and with conditioned response learning have been added, and that several of the chapters from the first edition have been consolidated or omitted. Thus, the three chapters on retention, the conditions of retention, and the conditions of forgetting have been combined into a single chapter. The material on reminiscence has been removed from this context and is now considered in the chapter concerning the distribution of practice.

In a similar way, the chapter on curves of learning has been dropped, portions of the material being assigned to other sections of the present volume. Interest in learning curves, as such, has been on the decline for many years and references to them are practically not to be found in the current literature. Other changes in organization have been of a less radical character, but may be found.

As far as content is concerned, changes that are not merely additive have also been made. Some of the material covered in the first edition has been omitted from the second. In other cases, it has been necessary to reinterpret some of the earlier findings in terms of the more recent experimental and theoretical knowledge we now possess. Perhaps the greatest change of content lies in the fact that a considerably greater amount of space has been devoted to a discussion of animal learning. Nevertheless, the book remains a psychology of *human* learning. Except in instances where their results clearly illuminate the process of learning in the human being, animal studies have not been included. Moreover, where several studies of animal learning appear to demonstrate approximately the same thing, only one or two of these are cited. This means that the present edition is in no sense a survey of the work on animal learning, since the references to this research are, in most cases, merely examples. In the same way, the chapter on conditioned response learning must be considered as an outline. So many of the concepts now employed in the explanation and understanding of human learning have been borrowed from the field of conditioning that it was felt necessary to include some systematic explanation of these concepts in the context of their origin. The vast literature of conditioning cannot be so casually summarized, however, and this chapter omits some of the more obvious facts and almost all of the subtle ones concerning this process. Indeed, the supporting evidence for the major findings in this area has not been cited in any detail. For a more detailed consideration of these subjects, the reader must be referred to the standard reference works in these areas and to the literature upon which these works are based.

A few words should be said concerning the literature with which

this book does deal. Articles on the learning process have appeared at an accelerated rate during recent years. During the decade that has elapsed since the first edition was completed, between two and three thousand articles have been published in this general area. In addition to these, a great number of articles dealing with closely related material have been published. This volume of publication represents, of course, a respectable proportion of the total research in the field of learning—about 30 per cent, I should judge. Such an enormous quantity of material makes selection imperative. It also has caused the percentage of coverage to decline. Certain systematic selection procedures were used. First preference for inclusion was given to articles that had been published in standard journals or books, second preference was given to abstracts of articles in such sources as the *Psychological Bulletin* and the *American Psychologist*, while third preference was given to articles and abstracts that had appeared in other sources less likely to be available to the student and the research worker, such as the proceedings of the various state academies of science, the publications of the armed forces, and the like. In addition, an attempt was made to credit priority of publication wherever possible. Thus, of two researches that showed approximately the same result, the earlier is more likely to be included in this work than the later. Beyond this, of course, an attempt was made to select references on a merit basis. This form of selection is necessarily dependent upon the judgment of the author, and it is recognized that another individual might have selected a rather different set of references. It is hoped that the selection of material is adequate, but judgment is fallible and what is judged to be of lesser importance today may, in the light of further knowledge, be judged of greater significance tomorrow. Failure of judgment in such matters is, no doubt, inevitable, particularly in view of the recency of much of the material. A word should also be said concerning the deadline for the inclusion of material. This varied for the different chapters because these were completed at different times. In no case, however, was a deadline set earlier than September, 1950, and for many of the chapters, January, 1951,

was the deadline. Beyond this, an attempt was made to insert later work into the chapters that had already been completed, but this could only be done incompletely, otherwise the process of revision should have become perpetual.

Throughout, an attempt has been made, whether for good or ill I cannot say, to maintain the systematic position taken by the first edition. This is not to say that all of McGeoch's decisions in such matters have been allowed to stand. Rather, an attempt has been made to estimate how he would have reacted to the developments that have occurred. This is, of course difficult to do and it is to be hoped that Professor McGeoch's many friends will not feel that I have needlessly distorted his psychology. In general, where the facts have indicated the need for it, revision has been made, but where two hypotheses have appeared to be equally plausible, I have tried to give preference to the one most consonant with his general systematic viewpoint. That, in doing this, I have frequently had to curb my own somewhat more violent behavioristic tendencies, I cannot deny. It has, however, been a useful, and I hope a successful, discipline.

Encouragement and aid have been received from many sources. Indirectly, my experiences as a student of Professors A. W. Melton, Kenneth W. Spence, and John McGeoch, himself, have been of inestimable aid. I have leaned heavily upon their teachings and I know that much that has been included in this book stems directly from their influence. To the authors who have permitted reproductions from their works and who, in some cases, have sent me manuscripts of their to-be-published research, I owe a debt of gratitude. Discussions with Professor G. R. Grice of the University of Illinois have served to clarify many problems, and his suggestions for this revision have been uniformly valuable and constructive. Several of the chapters were read in manuscript by my father, Professor Theo. W. H. Irion of the University of Missouri. His critical comments have been most helpful and many of his suggestions have been incorporated into the final manuscript. Finally, may I express the gratitude I owe to my wife who served as a shield against the clamoring of undergraduate student telephone calls and other distractions, who typed

the final manuscript, who aided me throughout in checking references and reading proof, and, not least important, who managed to tolerate my presence for the many days and nights during which this work was advanced.

A. L. I.

April 23, 1951
Urbana, Illinois

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CONTENTS

LIST OF FIGURES

THE PSYCHOLOGY OF HUMAN LEARNING

I

CONCEPTS AND METHODS

THE CONCEPT OF LEARNING

SYSTEMATIC experimental study of human learning dates from 1885 when Ebbinghaus stated some of its fundamental problems, devised methods for studying them, and in many ways set the pattern for later research. His monograph, *Memory: a contribution to experimental psychology* (1885, 1913), is a landmark in the history of psychology and is still a model which will repay careful study. Since that time, research in learning has appeared at an accelerated rate, and, with advancing knowledge, it has become increasingly clear that the concept of learning lies at the heart of psychology.

The work of Ebbinghaus was not, however, a unique event in the history of experimental psychology. The long history of associationism had preceded and conditioned his work, just as had the growing tradition of German natural science.[1] As Ebbinghaus' work was the product of his individual genius and the intellectual background of his time, so other minds combined with this background of thought to produce somewhat different experimental and theoretical approaches to the problem of learning. Thus, Thorndike's monograph, *Animal intelligence: an experimental study of the associative process in animals* (1898), set in motion another trend of thought which continues to be influential in the psychology of today. Similarly, the work of the Russian physiologist, Pavlov, on the conditioning of reflexes, although delayed in its incorporation into the body of psychological knowledge, has had an enormous influence in determining present-day thought in the field of learning.[2]

[1] The serious student, desiring to understand the background against which the early work in learning stands, would do well to consult Warren's *History of the association psychology* (1921) and Boring's *A history of experimental psychology* (1929, 1950).

[2] Pavlov's work became widely known in America only after Anrep's transla-

The Generality of Learning

Learning, which begins at birth and continues until the eventual disintegration of the organism, is a major developmental dimension of mind. A knowledge of its characteristics and of the conditions which determine its occurrence is fundamental to an understanding of psychological development and organization. The significance of the concept is widened by the fact that its relevance is not limited to organization and development as general problems in psychology, but extends into many special and applied fields.

The pervasiveness of learning can be grasped more clearly if one considers the part it plays in some of the categories of psychology. Psychologists have long recognized that modes of perceiving are functions of past experience, which is another way of saying that they are products of learning. Experimental evidence, accumulated through the last fifty years, has given this observation rich objective support. From simple perceiving of objects, through perceiving of spatial relations, to perceiving the most complex phenomena of whatever kind, learning is a dominant feature. The symbolic functions called ideas are learned, as has been recognized ever since the doctrine of innate ideas was discarded. The solutions of problems by means of ideas, called reasoning, exhibit the pattern of learning and are determined by similar conditions.

The specificity of likes and dislikes, indeed the whole range of affective and emotional response to stimulation, is mainly learned. Observations upon the development of personality traits have revealed that a great many—probably a majority—are specifically what they are as a result of learning. Many of the emotional difficulties which confront maladjusted personalities are the result of a learning process, as are the solutions to the problems which these emotional difficulties present. Even the motivating conditions which select and direct behavior are highly susceptible to learning and, as development proceeds, become increasingly learned. Relatively early in life, the native motivating conditions become so overlaid

tion of his book, *Conditioned reflexes: an investigation of the physiological activity of the cerebral cortex*, became available in 1927.

by learned modifications that the native conditions are present only by inference and history. The individual's sets, interests, attitudes, prejudices, wants, and beliefs are, thus, a product of his personal history and, in turn, are determiners of his present and future behavior.

If the reader will attempt to imagine the removal from his response-repertoire of everything that he has learned, he will be in a position better to understand the ubiquity of the learning process and its results in habitual behavior. When the results of learning are removed, little is left but the vegetative processes and a relatively few simple overt responses. Ability to recognize now familiar objects, to perceive the external world as one now perceives it, to read, to converse, to think of the things one now thinks about, to be moved to action toward and by the things which now move one, to respond in now accustomed ways to other people—these and many other activities have departed. Nearly everything, in fact, which makes man the complex psychological organization that he is, has gone.[3]

These statements about the pervasiveness of learning do not imply that nothing is inherited. The inherited structure of the organism is a basic condition of all learning; it sets the bounds and framework within which practice has its influence. The simple native responses and the primitive motivating conditions are, likewise, the starting points of learning, and the maturation of structure and function through the early years of life serves to complete the contribution of heredity to the conditions of learning. Heredity determines the nature of the receptors through which behavior is initiated, the nervous system, muscles and glands by means of which it exists, and, within this native framework of structures and events, learning occurs.

[3] The generality of the concept of learning has been treated in more detail elsewhere (McGeoch, 1936). Carr's textbook (1925) is a concrete demonstration, though without explicit assertion to that effect, of the way in which learning pervades general psychology. Hollingworth's (1928) organization of the field in terms of the concept of redintegration is a demonstration of the same point in a different way. Perhaps the most explicit statements of this point in the more recent literature are contained in Miller and Dollard (1941) and Dollard and Miller (1950).

The Problem of Definition

In science, as in logic, definition of concepts is of fundamental importance. However, whereas in logic definitions may be arbitrarily constructed according to formal properties, the only form of definition which has proven to be consistently useful in science is the *operational definition*. To be sure, the scientist is allowed a certain amount of arbitrariness in his definition of terms. He is allowed infinite latitude in the varieties, numbers, and complications of the operations he employs in definition. But this is his limit. He may not, without danger, go beyond the defining operations which he performs. Even when he defines the same concept by two different sets of operations, he is bound to construct transformation equations which must inevitably face the criterion of empirical validation. In the psychology of learning the definition of concepts has not always been accomplished with proper care, and defined concepts have not always retained their proper meanings. It is instructive to take the concept of learning, itself, as an example. Most definitions of learning make mention of the fact that learning consists of a change in performance. This change is definable in terms of two separate measurements of the behavior in question, the precautions and controls involved in making such measurements being of small concern to us here. The difficulty arises from the fact that most psychologists do not wish to classify all changes of behavior as learned. The student will immediately think of changes in behavior which he would prefer to designate as negative adaptation, motivational changes, fatigue effects, degenerative changes, changes produced by the maturation of the individual or by environmental variations, and so on. At this point, it is possible to define learning in either of two ways. On the one hand, we can define all changes in behavior which are *not* learned and define learning as the residual variability of behavior. On the other hand, we may take the more direct course and attempt to delimit precisely those behavioral changes which we desire to call learned. In practice, neither course has proven to be entirely satisfactory for the reasons that are outlined below.

An attempt to delimit those changes in behavior which are learned inevitably involves a statement of the conditions under which those changes occur, there being no other adequate source of delimiting criteria. This places the learning psychologist in a somewhat embarrassing position. On the one hand, he desires to study the conditions under which learning occurs and to establish functional relationships between these conditions and the learning process. On the other hand, he may not study the learning process at all (strictly speaking) unless he makes a preliminary statement of the conditions of learning for definitional purposes. Thus, in a certain sense, he has defined away a large portion of his area of study. Furthermore, it must be noted that, by defining learning in terms of the conditions under which it occurs, the psychologist commits himself to a certain theoretical and systematic position. Thus, learning defined from the standpoint of the reinforcement theorist is a somewhat different concept from learning as defined by the contiguity theorist. If the reinforcement theorist defines learning in terms of a change in behavior which occurs under certain conditions, one of those conditions being the occurrence of reward, he has forever settled the problem (for himself) of whether *learning* can occur in the absence of reward. It is true that changes in *behavior* may take place as a result of sheer contiguity, but these changes are not, under his definition, learned changes. It is apparent that, under these circumstances, there is a considerable opportunity for dispute among psychologists. Many of these disputes may be disguised as being concerned with factual aspects of the learning process. More often than not, however, these arguments actually concern definitions.

In view of these considerations, it is probably not worthwhile, so early in this work, to limit discussion by a rigid definition of the learning process. Probably, also, it is futile to discuss critically the various definitions which have been proposed. Instead, a general statement will be made which will include the phenomena discussed in this book. *Learning, as we measure it, is a change in performance which occurs under the conditions of practice.* What these conditions of practice may be, it is the purpose of this book to explore.

Learning and Retention

It has long been customary to divide the field into two main parts, "learning" or fixation, and "retention." At the level of logical analysis, this division is clear because an act must be fixated before it can be retained. In the learning of complex acts by continued practice, however, our measurements do not often separate fixation from retention, and the two are intermingled in each practice trial after the first. For the sake of convenience, the distinction between learning and retention has been given a different meaning than the logically analytic one, *learning* being used to designate the acquisition of changes in behavior during a specified time or up to a certain level, and *retention* being used to mean any measured persistence of these changes after practice ceases. Any failure of such persistence is called *forgetting.*

The way in which retention pervades learning, as these words are commonly used by psychologists, can be seen in any learning activity requiring more than a single trial, such as the learning of a series of words. The changes in behavior (verbal responses) acquired during the first trial are retained, at least in part, until the second trial. There, new ones are added to those retained; some or all of the results of the first and second trial are retained until the third trial, when more responses are added, and so on, until practice stops. If the results of the practice were not carried over from trial to trial, if they did not accumulate progressively, many trials would yield no more learning than would one trial, alone.

Not all of the acquisitions at each successive trial are carried over to the next; some are forgotten and must be refixated. A curve of learning represents a progressively greater balance in favor of retention, so that it is, in part, a retention curve. In addition to the initial modifications of behavior, which do not involve retention in the sense meant here, the retained modifications are further changed as they are repeated on later trials. Fixation and retention thus mutually interact in the course of what we call learning. It may also be stated briefly here, to be elaborated much later, that learning pervades retention in the sense that one of the conditions of forget-

ting is the learning of other responses. That is, one forgets by learning other things.

Usage has fixed certain names upon clusters of conditions. These names cannot mislead if one understands clearly what they mean. When studying the acquisition of behavior changes up to some arbitrary criterion, such as two perfect trials in succession, it is customary to speak of "learning" and to disregard in the naming the fact that retention has pervaded the process. Similarly, when performance is measured after an interval of no practice, it is customary to call the results measures of "retention," disregarding the fact that there would have been no learning to be retained had there been no cumulative retention of behavior changes during practice, and disregarding the fact that further learning has been one condition of loss, if any, in retention.

CHARACTERISTIC MATERIALS AND METHODS

A great many different varieties of experimental learning situations exist. Some of these are uniquely human, while others may be employed over a considerable range of other species. Conversely, some learning situations are best adapted for use in the study of learning among the lower animals. Concerning learning abilities, there are of course, wide variations within the animal kingdom. A consideration of these differences lies outside the scope of this book, being the proper concern of a work on comparative psychology. On the other hand, investigation has not revealed important differences in the type of learning between man and other animals. That is to say, the fundamental laws governing the learning process appear to be the same as we move from one species to another. To the extent that this is true, psychologists have received a distinct experimental advantage, for many aspects of the learning process may be more conveniently studied with the lower animals than with man.[4]

[4] This is an assumption which appears to be justified at our present state of knowledge, at least over a very wide range of species. Subsequent investigations may reveal that this assumption lacks generality and that, in some cases, the findings of animal experiments with a particular species have no applicability to the learning of other species, including man. This problem is considered in greater detail in Chapter II.

This is because the lower animals possess a less complex psychological organization wherein the effects of practice may be more unambiguously revealed. Furthermore, it is possible to control some types of experiment more adequately with animal than with human subjects. It is quite apparent that the effects of extirpation of nervous tissue, administration of drugs, certain diets, and of certain primary drive states may be more satisfactorily studied with animals than with men.

The variety of situations which have been employed in the study of learning are bewilderingly numerous, and a complete discussion of these situations would be of great length and small value. Some of these situations will be described in considerable detail in other sections of this book. It will be sufficient, here, to describe a few characteristic materials and methods.[5]

For experimental purposes, practice is usually administered under a rigidly controlled set of circumstances.[6] Among these circumstances are the instructions which are given to the subject. These instructions inform the subject (usually) of the kind of material he is to learn and whatever else the experimental problem requires and permits that he know. The instructions the subject receives serve to direct and control his activities during the experimental period and are, thus, one of the basic experimental conditions. In studies of learning among the lower animals, verbal instructions, of course, cannot be given. Instead, other means are employed to direct the animal's activities in a particular way. For example, the animal may be placed in a restraining harness or in an enclosed maze or problem box. These restraints serve to direct and to restrict

[5] Melton (1936) has published a thorough examination of methodology in experimentation upon learning. More recently, Hilgard (1951) has written an excellent survey of methodology in the study of learning. In the present book, the emphasis is primarily upon the results of research rather than upon the methodology of research. For the latter, Melton and Hilgard should be consulted.

[6] This rigidity of control has led certain individuals to complain that learning, as it is studied in the laboratory, has little to do with "real-life" situations and that the conclusions derived from such experiments are artificial and invalid. This attitude represents an almost complete misunderstanding of the scientific method. Life in the laboratory is just as "real" as life may be anywhere else. It is, however, more adequately controlled.

the animal's activities in much the same fashion as do the instructions which are given to human subjects.

Before actual experimentation begins, a certain amount of pre-training is often given. These practice periods serve to acquaint the subject with the general nature of the learning problem and, it is thought, tend to reduce the extent of individual variations in the performance of the learning task. In some cases, of course, the problem under investigation is of such a nature as to make such practice periods undesirable features of the experimental design.

Following the pre-training period, practice is administered to the subjects under the various conditions of the experiment. These conditions typically include one or more experimental conditions and at least one control condition which serves as a standard of comparison against which the experimental condition results may be evaluated. In some problems, a formal control condition may not be necessary. For example, in an experiment to determine the relative effectiveness of two methods of learning the same material, each experimental group may serve also as a control group, that is, as a standard of comparison for the other group.

The division of subjects into experimental and control groups introduces some of the most perplexing problems of experimental design. It is worth while to consider some of the methods which have been used to accomplish this.

(A) *The method of the counterbalanced practice order.* In this method, each subject serves under all of the conditions of the experiment. In an attempt to cancel out the systematic error which would be introduced by transfer of training and other factors if each subject went through the experiment in the same order of conditions, a different order of going through the conditions is assigned to each subject. In the most simple case where two conditions, A and B, are involved, half of the subjects are assigned to go through the conditions in the order AB while the other half go through in the order BA. The division of subjects into these two groups may be done at random (by coin-tossing, drawing from a table of random numbers, etc.), or, in case some basis for matching the group exists, an attempt may be made to equate the groups. When more than

two conditions exist, the task of counterbalancing becomes more complex. Not only should each condition appear an equal number of times at each position in the counterbalanced practice order, but it may also be desired to have each condition precede and follow each other condition an equal number of times. This type of counterbalanced practice order is illustrated in the diagram, below. In this diagram, four conditions, A, B, C, and D, are arranged so that each condition occurs once in each order, and also so that each condition precedes and follows each other condition once. This block of four subjects may, of course, be repeated as many times as are desirable in order to increase the number of subjects.

Subject	Order of Conditions			
number	1st	2nd	3rd	4th
1	A	B	C	D
2	B	D	A	C
3	C	A	D	B
4	D	C	B	A

The great advantage of using a counterbalanced practice order lies in the fact that it is possible to conduct an experiment containing a large number of conditions with a relatively small number of subjects. There are, however, serious drawbacks to this type of design. The most serious of these is the assumption, which is implicitly made, that the interaction between conditions and position in the counterbalanced practice order is, on the average, zero. If two conditions of an experiment show a different susceptibility to the influence of position in the counterbalanced practice order, the purpose of that counterbalanced order is defeated and a more or less serious source of error is introduced. Examination of the data from experiments of this type will often reveal the presence of this source of error. Although this effect may be more serious in some types of experimentation than in others, it is probably not safe to use counterbalancing unless it may be shown that interaction between the conditions and the position of those conditions in the

counterbalanced practice order is not significant for that particular type of problem.

(B) *Separation into randomly selected groups.* Under this plan, the entire group of subjects is divided at random into a number of sub-groups. Each of these sub-groups is then tested under a single condition of the experiment. This design does not contain a source of systematic error of the type mentioned above. Furthermore, such a scheme presents the experimenter with an opportunity to make a relatively simple statistical analysis of the results he obtains. The disadvantage of this design is that it requires a large number of subjects if the number of conditions is very great.

(C) *Separation into equated groups.* In order to increase the precision of the experiment, it is often possible to separate the subjects into equated rather than into randomly selected sub-groups. In order to do this, of course, some basis for equating the groups must exist. This may be done by means of a pre-test score, or it may be done by stratifying the sample according to some previously measured variable which is thought to have an important bearing upon the results to be obtained. One of the most efficient and powerful methods of accomplishing this is the technique known as analysis of covariance. Under this method, the subjects are separated into *randomly selected* sub-groups, but the pre-test scores (or other scores to be used in the equation of the groups) are used in the final analysis of results in such a way that initial, chance differences between the groups are taken into consideration. This method is often more efficient and convenient to use than a method which involves the selection of actually equated groups in the beginning of the experiment. It possesses the further advantage that it does not violate any of the assumption necessary to the statistical analysis of the data, an advantage which is not shared by some of the traditional methods used in the actual matching of groups.[7]

All learning experiments necessarily involve the learning of some-

[7] A detailed consideration of statistical methods (including analysis of variance and covariance) and their applications to the experimental study of learning lies outside the scope of this book. The student is referred to any recent book on psychological statistics such as the one by McNemar (1949).

thing—some new type of performance must be acquired, or an old type of performance must now occur in situations which previously did not elicit that type of activity. Things learned may be called either materials or activities. Considered from the side of the situation which elicits the changes in performance called learning, they are materials; considered from the side of the performances observed and measured, they are activities. It will be convenient to use these terms interchangeably.

In studies concerned with human learning, certain types of materials and activities have traditionally been used. A brief discussion of some of the more important types of these materials is offered here in order that the student may be able better to understand the experimental results which are presented throughout the remaining portion of this book.

Verbal Materials

In selecting verbal materials for many experimental purposes, it is desirable to have items which are unfamiliar to the subject in order to avoid the complex associative connections which may already exist among familiar materials. If the materials are unfamiliar to all subjects, these subjects will begin practice more nearly on an equal basis. One of the standard verbal materials is the *nonsense syllable*, first used by Ebbinghaus. A nonsense syllable consists of a vowel between two consonants, such as DOQ, ZEH, or XAB. It is unlikely that a subject will come to an experiment with already formed associations between such syllables as these. The syllables will, however, suggest some meaningful associations to a subject, and they will differ among themselves in terms of the number of associations they arouse. They are "nonsense" syllables only in the sense they have no accepted meaning in any language known to the subject. Few of them are "nonsense" if one means by that term a total lack of suggested associations.

Glaze (1928) calibrated nonsense syllables in terms of their associative value by exposing one syllable at a time to each of fifteen subjects and allowing three seconds for the arousal of an association,

if any. The syllables were then classified in terms of the percentage of the fifteen subjects having associations aroused by them. Only 101 of the 2019 syllables which were studied in this way suggested no meaning to any subject, i.e. had 00 per cent associative valued for these subjects, while only 119 syllables had a value of 100 per cent. The remainder were distributed between these two extremes. The syllables QIJ and ZYQ are illustrations of the 00 per cent class, while NOV and VIK are examples of the 100 per cent class. Other methods of calibration, such as Hull's (1933) method of asking the subjects to report meaning suggested by the syllables during learning, give somewhat different results. The important thing is that all of the calibrations yield groupings of the syllables according to the associations or "meaning" aroused by them. Few nonsense syllables suggest other nonsense syllables, however, so that no part of the order of a series of them is likely to be already partially known to the subject; but most nonsense syllables suggest meaningful associations to at least some of the subjects, thereby aiding (or hindering) learning through transfer of training. The calibration of nonsense syllables in terms of the actual difficulty involved in learning them has not been undertaken on a large scale. The results obtained by Hall (1950), however, suggest that the difficulty of a particular syllable depends, in large part, upon the difficulty of the other syllables which are being simultaneously learned in the same list.

A related kind of verbal item is the *consonant "syllable,"* consisting of three consonants, such as CNR and QMX. Witmer (1935), who introduced the consonant syllable, has calibrated 4534 three-letter consonant syllables constructed from nineteen consonants. This calibration, again, was in terms of the associations suggested by the consonant syllables. Consonant syllables possess an advantage over the conventional nonsense syllables because they are less meaningful and because there are more of them. Most of the consonant syllables studied by Witmer, however, aroused some association in her group of twenty-four subjects. The items ZQJ and XFQ were among the eleven having 00 per cent associative value, while the items BRD and WHP were among the ten having 100 per cent

value. The remainder were distributed over the intermediate values in a way which approached the normal distribution curve.[8]

Many other types of material have also been employed in studies of verbal learning. Among these are lists of digits, whether singly or in groups of several per item; lists of meaningful words (chosen, usually, to make homogeneous lists, as lists of two-syllable adjectives or of four-letter verbs) or sections of poetry or of prose. The calibration of these materials for use in verbal learning experiments has not been as extensively accomplished as in the case of nonsense and consonant syllables. This is partially due, no doubt, to the fact that these materials are less amenable to this type of measurement. Melton and Safier (1936) scaled the meaningful similarity of 300 pairs of two-syllable adjectives. More recently, Haagen (1949) has outlined a more elaborate procedure for the calibration of word pairs.

Verbal materials may be presented to the subject in many ways, and the influence of practice may be variously measured. In the *anticipation method*, the items of a list are presented in the window of an electrically driven (or other type) exposure apparatus, one at a time, and at a uniform rate. The subject is instructed to connect each item with the one following it, so that as each appears in the window he can anticipate the next one by speaking it aloud before it is exposed.[9] This method has a number of advantages. The presentation time per item, and hence per repetition of the total list, is controlled. Number of repetitions can, therefore, be converted into time, if one wishes. Each trial is both a presentation and a measure of learning, since, after each attempted anticipation, the correct

[8] Witmer (1935) cites and discusses other calibrations of learning materials, and compares her own methods and results with those of others. Her paper contains a list of the 4534 consonant syllables, grouped according to associative value. Similar groupings of nonsense syllables may be found in Glaze's (1928) paper.

[9] The best current experimental practice, when using either nonsense or consonant syllables, is to require the subject to spell the syllables rather than to pronounce them. The syllable, XED, for example, when pronounced, may easily be mistaken for ZED or SED, and a large number of similar confusions are possible. The spelling of each item permits the experimenter to make a more accurate record (including part scores) than does pronouncing.

item is exposed to the subject. The method permits a record of the course of acquisition of each individual item, thus yielding data for the plotting of a learning curve either for the entire list or for each of the component items.

The *paired-associates method* involves presentation of pairs of items, as in a vocabulary list, under instruction to learn to associate the two members of each pair so that, when the first is presented the second can be recalled. These pairs of items are usually presented on a memory drum so that the first member of each pair is exposed alone, followed, after a short interval, by the simultaneous presentation of both members of that pair. The paired-associates method thus resembles the anticipation method, except for the presentation of the items in separate pairs, and for the fact that the pairs are usually presented in different serial orders upon successive trials. This change of order is necessary to prevent the subject from learning the second items of each pair as a series of responses rather than learning to associate them with the first items in each pair.

Occasionally, the *method of complete presentation* is employed, although it has several disadvantages. All of the material is presented for a certain period of time. There is no control over the presentation time of the various parts, as when one reads a chapter at one's own rate, spending little time on some sections and much more on others. Learning is then tested by recall. This method permits no control of the way in which the subject distributes his time over the various parts of the whole, and it gives no measure of progress, except the single measure of recall at the end. Usually a time limit is set, but sometimes the subject is instructed to study the material until he has reached some criterion of perfection, such as one perfect recitation. The latter procedure is especially inadequate, since subjects will differ widely in the points during study at which they will attempt a recall, and in the amounts of time spent on such attempts. One subject may attempt recall before a complete recall is possible, while another may be overconservative and wait until the material has been learned well beyond the threshold of one correct recall.

The variant forms of these methods will not be described. Of the other methods, only the *memory span method* need be mentioned. By that procedure, the experimenter presents to the subject lists of digits, consonants, or words. These are presented at a constant rate of speed, say, one digit per second. The number of items presented on a single trial will vary from around four to, let us say, twelve, a different series of items being presented on each trial. The score, called the subject's memory span, is that length of list which can be recalled perfectly after a single presentation on a specified proportion of the trials, say, 50 per cent of them.

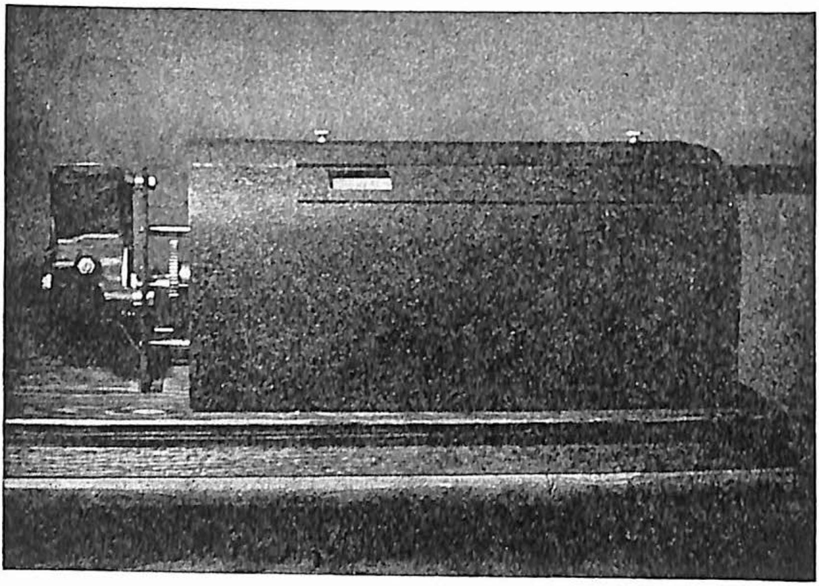

FIG. 1. A TYPICAL EXPOSURE APPARATUS FOR PRESENTING VERBAL MATERIALS
The materials to be learned are typed or printed on a strip of heavy paper or cloth which is then fastened around the drum. The drum moves one space every two seconds (or other desired interval) to bring another item or section of material into the window.

Perceptual-Motor Activities

In experiments on learning, the term, perceptual-motor, refers to the learning of associations between stimulating conditions and overt motor responses which are not primarily verbal. In view of the extent to which the conditions of human behavior are pervaded by

verbal symbols, this distinction can be only a rough one at best. The term, skill, is often used instead, and can be used justifiably for many activities. Either term is no more than a conventional name for a group of activities which are not verbal, alone.

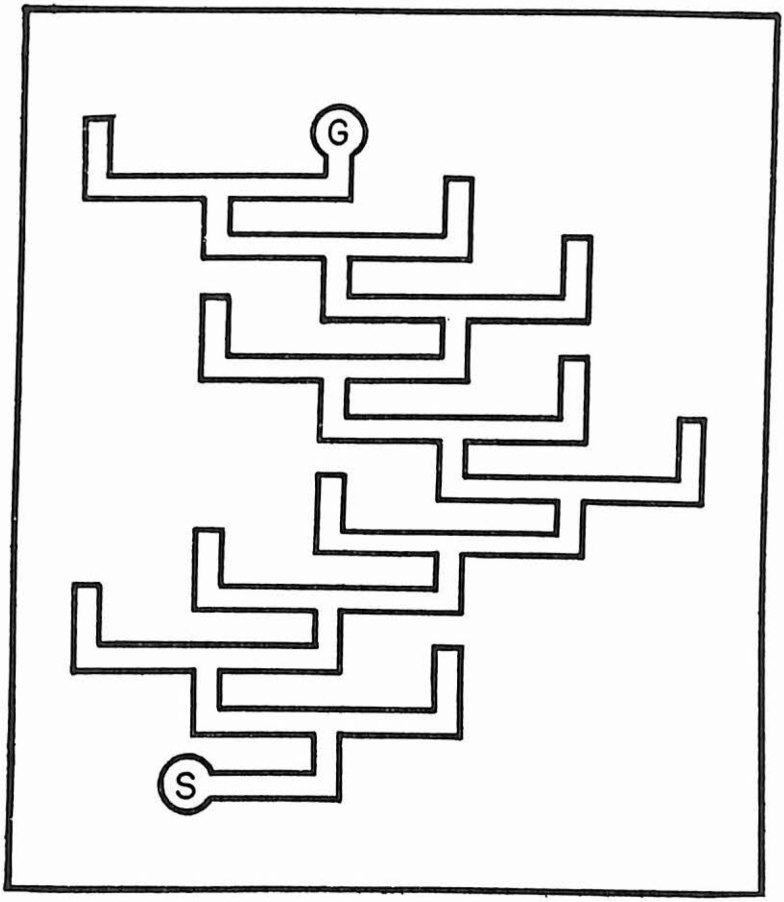

FIG. 2. A STYLUS MAZE PATTERN
(From Warden, *J. exp. Psychol.*, 1924, 7, p. 101)

The *maze* is a frequently used instrument for studying perceptual-motor learning. Mazes may be constructed in a variety of ways, but they have the common feature of a true path from start to goal and either one or more—usually more—blind alleys branching from the true path. The paths may be actual paths through which the sub-

ject walks, but more typically they are cut from brass, fiberboard, or other material, in which case the subject traces them with a stylus. In any event, the subject is instructed to find and then to fixate the true path without the aid of vision. Practice effect in maze learning is commonly measured by trials, time, and errors in reaching a criterion.

Fig. 3. A Pursuitmeter (or Pursuit Rotor)
The jointed stylus is lying on the turntable.

The activity required in learning to operate a *pursuitmeter* is advantageous for the study of learning because it is only slightly susceptible to verbal control and cannot be learned as a verbal series. This, of course, is not true of the maze learning situation. Maze patterns may be learned as a series of verbal self-instructions to "go straight ahead," "turned right," and so on (cf. Warden, 1924; Husband, 1928). A pursuitmeter consists of a target moving at a relatively high rate of speed, in some cases with a regular and constantly repeated pattern of transit, in others with an irregular and variable pattern. The instrument shown in Figure 3 has a smooth turntable into which is set a brass target button, near the periphery and flush with the surface of the turntable. The turntable is revolved by a synchronous motor through a system of gears which permits speeds of 30, 60, and 120 r.p.m. in either direction, clockwise or counterlockwise. The subject is to pursue the target button with a jointed stylus, his learning being measured by the total time spent in contact with the target during each practice period.

During and since the recent war, a great number of such mechanical perceptual-motor learning situations have been developed. Some of these are completely new in design while others are modifications of previously existing instruments. Characteristic of such devices are the ones described by Lewis and Shephard (1950) and Shephard and Lewis (1950).

The nature of other activities in this general group can be sufficiently understood from their names. Typing, telegraphy, tracking, mirror-drawing, card-sorting, ball-tossing, and target-shooting are examples. The specific methodology used will vary with each one, as will the units in which learning is measured.

Activities Which Primarily Involve Responding to Relationships

There is another group of activities which have come into prominence more recently than the others. The distinguishing feature of these activities is that they require the response to relationships and principles. A great many learning materials and activities involve this, but in a somewhat less pronounced degree.

Multiple choice problems which can be solved by the discovery and application of a generalization or principle are an example of this type of learning situation. Let the subject be presented with a number of choices, as keys to be pressed, and told to find which one is the correct one, as indicated by some signal. When he has found the correct choice, he is next given a different number of keys and again required to find the correct one. The correct choice is always determined by some principle, such as alternation from right to left end, or a more difficult one, such as second from the left, first on the right, middle, and repeat. The subject must discover the principle which will permit him to solve the problem each time, regardless of the number of keys.

Other forms are the so-called *Umweg* or *detour learning* situations in which the direct path to a goal is blocked so that the subject must take an indirect route in order to solve the problem, the simple *learning of relationships* in which the subject must discover that the problem can be solved by response to some relation such as "darker than" or "longer than," and innumerable abstract problems of a mathematical and conceptual character.[10]

Methods of Measuring Learning and Retention

The measurement of learning depends upon our ability to identify the changes in behavior which we desire to call learned. Traditionally, a number of different measures of learning have been used. These measures may be classified according to their complexity. It is recognized, of course, that other methods of classifying measures of learning may be devised.

(A) *Direct measures of response.* Degree of learning may be measured by making a direct measurement of the learned response. All of the measures classified under this heading may be applied to the performance of a single subject on a single trial or test-period. Three such measures exist. These are:

[10] A theoretical controversy of considerable magnitude is being waged concerning the nature of this type of learning. A brief discussion of this controversy may be found in Chapter IX.

(1) *Response amplitude.* This measure may be applied only to certain types of learned behavior. It is one of the most frequently employed measures of learning in the study of conditioned reflexes. Amplitude of the conditioned knee-jerk reflex, number of drops of saliva secreted upon presentation of the conditioned stimulus, and amount of change in skin resistance (in the conditioning of the galvanic skin response) are examples of the use of this measure.

(2) *Response latency.* All learned responses are elicited by stimuli. In some cases, the experimenter controls the stimulus which elicits the learned response, and, in such cases, he can measure the lapse of time from the onset of this stimulus to the occurrence of the learned response. Whether or not the resulting latency measure will have meaning depends upon the nature of the learned act, for in some cases the conditions of learning are such as to train the subject to wait a specified period of time before responding to the eliciting stimulus, *i.e.*, in the case of delayed and trace conditioned responses.

(3) *Response accuracy* or *quality.* This measure of learning is often employed in studies of human learning. In perceptual-motor tasks, particularly, this measure is frequently used. Thus, in learning to aim and fire a rifle, we are interested in the distance from the bullet hole to the center of the bull's eye. Similarly, a measure of accuracy underlies such simple derived response measures as "time on target" in pursuitmeter learning.

(B) *Simple derived response measures.* Measures classified in this way are those which require more than one subject or more than one test-period, or both, in order to be applied. There are a number of such measures in common use. These are:

(1) *Frequency.* Measures of frequency of response can be applied whenever the occurrence of the learned behavior can be dichotomized in an all-or-none fashion. In order for the measure of frequency to be employed, it is necessary that there be multiple subjects or multiple trials with a single subject. Thus, we may measure learning in terms of the proportion of a group of subjects exhibiting a learned response on a particular test trial, or we may measure learning in terms of the proportion of times a single sub-

ject makes the learned response during a series of test-periods. The measure of frequency may be applied to the learning of lists of words, the occurrence of conditioned responses, or in any of a variety of other learning situations. Furthermore, the measure of frequency can be applied to the occurrence of errors as well as to the occurrence of correct responses. Thus, in the learning of a maze, frequency of entrance into a blind alley can be used as a measure of learning in the same way that frequency of choosing the correct path can be so used.

(2) *Resistance to extinction.* Conditioned response learning exhibits the phenomenon of experimental extinction. This phenomenon involves the disappearance of previously established conditioned responses under conditions of non-reinforcement. The number of non-reinforced trials required to produce a specified decrement in a conditioned response may be used as a measure of the strength of original conditioning.[11]

(3) *Resistance to forgetting.* This measure is closely related to the previous measure. It rests upon the fact that well-practiced habits are less susceptible to forgetting than are poorly learned habits. Since, however, the process of forgetting is, itself, a product of activities occurring between the time of original learning and the measurement of retention, this measure is best employed only when these intervening activities are under the control of the experimenter. The use of this measure of learning assumes that some measure of retention and forgetting exists. There are a number of such measures. Three of these will be discussed under this heading, while the fourth, being more complex, will be described under the heading, complex derived response measures, below.

[11] In a recent article, Brogden (1949) calls into question the validity of this measure on the basis that he found no evidence of a correlation between the rate of learning and the rate of extinction in a conditioned avoidance training situation. Brogden erred, however, in training all of his animals to the same criterion of performance before starting extinction. Rate of extinction should be closely related to rate of acquisition only if original training is carried out to a trial criterion so that different rates of acquisition may be reflected in different levels of terminal habit strength. Due to this, and a number of other technical flaws in his experiment, Brogden's results cannot be considered to have demonstrated the inadequacy of this measure of learning.

(a) In the *method of recall* (or *reproduction*) the subject is given the original stimulus or some substitute for it and is asked to perform as nearly as possible in the same way as he did at the attainment of the criterion. Depending upon the activity learned, he is placed in the original experimental context, given the original instruction or cue, and asked to recall independently without the benefit of further aid. In verbal learning by the anticipation method or by the paired-associates method, the recall trial is a continuation of learning, and the measure of retention is continuous with the measures of learning. In a maze, or other type of problem, the subject is again confronted with the problem and asked to solve it.

(b) In the *recognition method* the subject is presented with the originally learned material mixed in a random order with other similar items, and is asked to select, from the total display, the items which were in the original list. The results are usually corrected for chance, since even without any retention a subject could make a fairly high score by chance. The method of recognition is limited to materials which, like verbal lists, are composed of readily separable parts.

(c) The *reconstruction method* has a similar limitation. At some time after he reaches the criterion, the subject is given the parts of the original material, arranged in a random order, and is asked to reconstruct the original order.

(C) *Complex derived response measures.* These three measures represent a still greater degree of complexity. In fact, they are not entirely separate measures, but actually may employ any of the measures previously discussed, but only under certain special conditions. The measures are:

(1) *Amount of practice required to reach some criterion.* In connection with some simple measure of response, this is a frequently used measure of learning. The amount of practice necessary to achieve some standard of perfection, such as two successive errorless repetitions of a list of nonsense syllables, may be measured in terms of trials or time. While, of course, this method does not measure amount of learning, as such, it is frequently employed to assess the effectiveness of a particular method of practice, for the

purpose of matching groups or individuals in terms of learning ability, and in connection with the savings method of measuring retention which is described below.

(2) *Resistance to forgetting by the savings method.* This method, first used by Ebbinghaus (1885, 1913), requires that the subject relearn the material, usually to the same criterion as was used in original learning. The saving is the difference between the measures of learning and relearning. These measures may be almost any of those which have previously been discussed, such as time taken to learn, number of errors made in learning, mean accuracy of response, or number of trials taken to reach the criterion. Usually, a percentage of saving is computed in terms of this difference divided by the corresponding measure of learning. Thus, if a subject requires thirty trials to learn an activity and ten trials to relearn it to the same criterion, he has saved twenty trials or 66.67 per cent of the original trials. The savings method has characteristics which other methods lack. It may reveal retention where the recall method shows none; it may yield negative savings scores as well as positive ones and is peculiarly able, thus, to serve as a measure of the influence of inhibitory conditions.

(3) *The transfer of training method.* This method may be used either to measure learning or retention. Any method for measuring the influence of practice at one activity upon the rate or other characteristic of the learning of a second activity is a transfer method. Let a group of subjects learn maze A and then learn maze B. If maze B is learned more slowly or more rapidly by this group than by an equivalent control group which learned B without having previously learned A, there has been a transfer effect from the learning of A to the learning of B. Since practice at A could not have influenced the rate of learning of B had not the results of practice been retained, the transfer method may also serve as a measure of retention. When being used as a measure of retention or of learning, and when being studied as a separate phenomenon, the transfer method may employ any of the common measures of learning which have been discussed previously.

A question may properly be asked concerning the reliability and

validity of these measures. About reliability, little in the way of a general statement may be made since reliability will vary according to the learning situation which is employed, the type of subject used, and the precision of control exerted by the experimenter. The reliability of a particular measure under a specified set of experimental conditions may, of course, be determined. That there are complications in even such a straightforward procedure as this is indicated by the analysis of Melton (1936).

Concerning the validity of these measures, an important question may be asked. Since typical correlations between the measures are far from unity, we may ask which measure is the "best" measure of learning, that is, which measure is the most valid. An answer to this question depends, in part, upon the definition of learning itself. If learning is defined in terms of response changes, then a particular measure of response change might be specified as being definitive. The definition of learning would then include a statement of the operations involved in making this particular measurement. Under these circumstances, other measures of "learning" would be valid only to the extent that transforming equations could be worked out which would convert these other measures to the terms and units of the basic measurement. On the other hand, if learning is defined in terms of stimulus events, then it is necessary that the relationships between these stimulus events and the measures of response be specified.[12] This may be accomplished through a system of intervening variables or connecting equations of some type. Validity of a response measure may now be specified in terms of the consistency with which changes in it may be predicted from a knowledge of the stimulus events.

[12] As has already been indicated, learning may be defined in terms of behavioral changes which occur under certain sets of conditions or stimulus events. Thus, Hull (1943) defines his intervening variable, habit, in terms of stimulus events such as amount and delay of reinforcement, number of reinforcements, and the temporal relationship between the eliciting stimulus and the to-be-learned response.

CURVES OF LEARNING

When more than one trial is required for learning, we often wish to know the relationship between the effectiveness of a particular trial and its ordinal place in the total series of trials taken to learn. This relationship may be quickly determined by plotting a curve of learning. Trials, or some other measure of practice, are plotted on the x-axis, and the corresponding measures of performance on the y-axis. The particular units plotted determine whether the curve will rise or fall. Curves for any measure of positive performance, such as frequency of correct responses, typically rise as practice increases. On the other hand, curves for latency measures or curves for frequency of errors typically fall with increasing practice. The important thing about a learning curve is not its tendency to rise or fall, but its form, which shows the relative influence of successive trials.[13]

Whenever possible, a curve is plotted for the entire practice period from its beginning until the attainment of the work or performance criterion. Such a curve represents the amount learned. In some cases, however, a curve of total performance would be difficult to obtain, or too cumbersome to be of value. In the more complex activities, such as typing or telegraphy, it is customary to measure the results of practice at sample points along the continuum of practice. The number of words typed in a ten-minute test at the end of every five hours of practice, or a complete record of words typed in every fifth hour of practice, may be used instead of the records of the entire period. This method is usually used, also, in conditioned reflex experiments since, in many studies of this type, it is difficult to obtain measures of learning during the actual practice periods.

There is no single curve of learning which can be called *the* curve of learning. Different tasks, experimental procedures, methods of measurement, and types of subject will yield different forms of

[13] A curve of learning is the line of the regression of performance upon practice. Practice is the known variable, performance as a result of practice, the unknown.

learning curves. However, most of the curves approach one of the three forms shown in Figure 4. Curve A, which is *negatively accelerated,* shows increments of performance which are relatively large early in practice and become smaller as practice continues. Curve B, which is *positively accelerated,* pictures increments which are slight during the early trials, but which become progressively larger. Curve C, which is an S-curve, begins with initial positive acceleration, passes through a region where linearity is approached, and then becomes negatively accelerated. Occasionally, a learning curve is obtained which is linear, or nearly so, for a considerable proportion of its course. These three curves are smoothed and

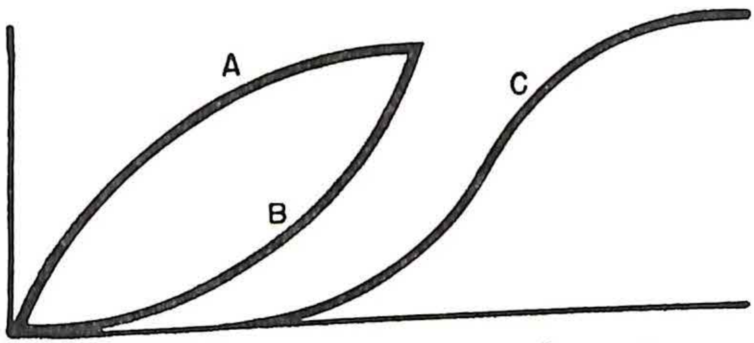

FIG. 4. REPRESENTATIVE CURVES OF LEARNING
(Curves A and B from Carr, *Psychology*, p. 218)

schematic, representing the ideal forms approached by experimentally obtained data. Curves for single subjects are much more irregular than this. In Figure 5 is drawn the learning curve for a single practiced subject learning a list of ten meaningful words to a criterion of two perfect trials in succession. This curve is less irregular than many, but it is still far from being smooth. It will be seen that, despite these irregularities, its form approaches Curve A in Figure 4. The occurrence of these up-and-down fluctuations in individual learning curves is a function of a number of "chance" conditions which the experimenter has not controlled, such as distractions, fluctuations of motivation, and temporary interferences among the parts of the activity being learned. When a number

of curves obtained under the same experimental conditions are combined, the chance fluctuations come at different points for different subjects, and the combined curve approaches smoothness.

Learning curves such as these typically approach some high value as a limit. Often this limit is determined by the nature of the task and the measures of learning used. Thus, when frequency of correct responses is used in an experiment which involves the learning of a

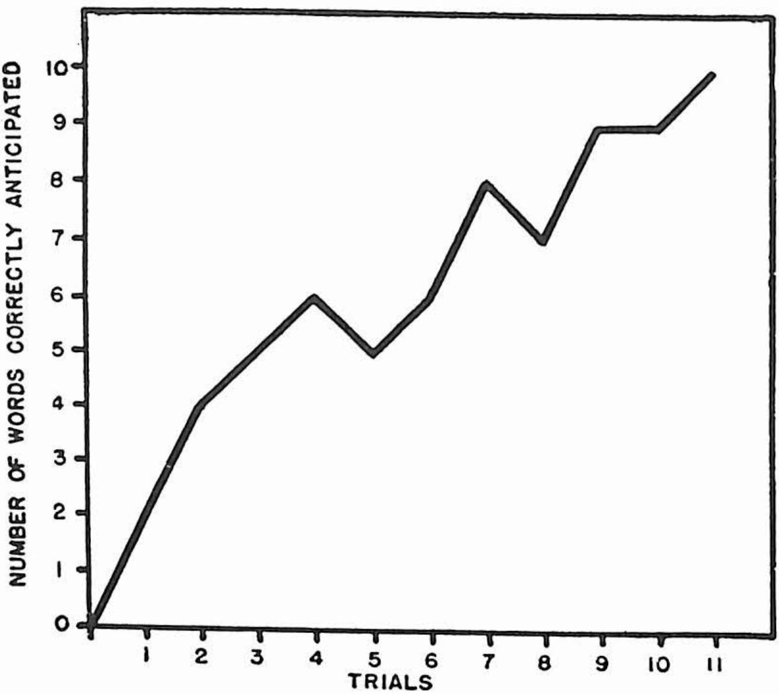

FIG. 5. CURVE OF ONE PRACTICED SUBJECT FOR LEARNING A LIST OF 10 WORDS

list of ten words, the learning curve approaches the value of 10 as a limit. Such limits are artificially imposed. But even when theoretical improvement is infinite, learning curves do not show indefinite rise. The conditions under which learning occurs set definite limits to the total amount of improvement to be derived from practice. When conditions for learning are optimal, we may speak of the final leveling off of improvement as an approach to the physiological limit.

Occasionally, learning curves show marked deviations from one of the forms shown in Figure 4. One such deviation is the occurrence of one or more plateaus during learning. A plateau is a period of little or no change in performance which is preceded and followed by periods of improvement. Another form of deviation from the more usual form of learning curve is the occurrence of a rapid period of improvement representing the more or less sudden solution of the learning problem. Such periods of sudden solution have been used as the criterion for, and the definition of, insight. The conditions under which such variations as these take place, as well as the conditions which determine the rate and limits of improvement, form the proper subject for a large portion of this book.

Vincent Curves

When learning is carried out to a performance criterion, curves for individual subjects cannot be combined by adding the numerical values directly. Subjects require different numbers of trials to reach a common criterion, and if we were to add the records for trials 1, 2, 3, ... *N*, the number of subjects represented in each total would decrease from the number of trials taken by the fastened subject onward, until, at the last trial, only the slowest subject would remain. If three subjects take 5, 8, and 10 trials, respectively, to reach a criterion, all will be represented in the first five totals, two in the totals for trials 6, 7, and 8, but only one in the totals for trials 9 and 10.

The problem can be solved for many purposes by *summating the amounts learned by different subjects in equal fractions of the total learning period*. The procedure for doing this, originally developed by Vincent (1912) and modified by Kjerstad (1919), Hunter and Yarbrough (1917), and others, involves a division of the total trials into fractions, such as sixths or tenths, and a determination of the performance of each subject in each fraction. The details of the procedure vary with the measures employed, and a critical discussion of the various procedures may be found in Hilgard (1938). We shall present here, as an illustration, the application of one of them

to the records of two subjects for the learning of a list of ten words by the anticipation method.

The two subjects required twelve and eight trials, respectively, to reach the criterion of two perfect trials in succession, but the numbers of correct anticipations in each trial, shown below, stop at the first perfect trial, because the following trial was also perfect, and only the first of the successive criterial trials need be included in the curve.

Subject A: 0, 2, 4, 5, 6, 5, 6, 8, 6, 8, 9, 10.
Subject B: 0, 3, 5, 7, 7, 8, 8, 10.

These data should be plotted on an abscissa which is of the same length in the two cases. The inevitable zero score at the first trial is plotted at the point of origin, the length of the constant abscissa is divided by the total number of trials minus one (11 and 7), and the corresponding positive scores plotted at the indicated points. That is, the value 2 for Subject A is plotted at a distance from zero which is one-eleventh of the total length of the abscissa, the value 4 at the next eleventh, and so on. For this purpose tables prepared by Melton for dividing a 200-millimeter abscissa into the fractions required by any given number of trials are useful. Once the individual curves have been drawn, the data for a Vincent curve are obtained by erecting at any desired fraction of the abscissa a perpendicular which intersects the curve and by reading the value on the ordinate at the point of intersection.

The Vincent values obtained in this way are the same as those arrived at by arithmetical computation, and are as follows for the two sets of values when cut at successive fifths:

Subject A: 4.2, 5.6, 7.2, 7.6, 10.0
Subject B: 3.8, 6.6, 7.2, 8.0, 10.0

The data for these two curves have been divided into fifths because, in order to avoid large errors from interpolation, it is advisable to use a number of divisions which is no larger than the smallest number of trials taken by any subject. Had subjects with larger numbers of trials been chosen for illustrative purposes, the resulting curves might well have been cut at successive tenths.

Vincent curves may also be plotted as Melton (1936b) has done, in terms of the number of trials (or other values on the x-axis) required to attain successive levels of performance. For certain purposes (cf. Melton, 1936; Hilgard, 1938) this is the more satisfactory method.

Learning Curve Equations

Learning curves are geometrical devices which express, in easy visual form, the relationship between practice and performance. A mathematical expression for any type of curve may be obtained. Such equations provide a terse, symbolic shorthand for the more cumbersome curves. Equations for learning curves may be divided into two general types, rational and empirical. In the latter type, an attempt is made to find the mathematical expression which will best "fit" a group of obtained data. Such empirical equations do not represent a great advance over the mere drawing of the learning curve for these data, although it may often be convenient to obtain an equation fitting data which have been obtained. Rational equations, on the other hand, do represent an advance in knowledge because an attempt is made to give meaning to the constants of the equation in terms of quantitative variations in the conditions of practice. Rational equations, therefore, are really quantitative hypotheses concerning the nature of the learning process since they specify precise relationships between the independent and the dependent variables of the learning process. If an equation of this type yields a close fit to experimental data obtained under a number of variations of the conditions of learning, we must regard the hypothesis implied by the equation as being verified to that extent. It must be noted, however, that the constants in such a rational equation must be determined by the nature of the experimental conditions and not be the arithmetical manipulations of curve-fitting. No rational equation has been devised which satisfactorily predicts and fits all learning curves. The development of such an equation would, of course, represent complete knowledge concerning the learning process. For data obtained under a restricted range of experimental conditions (*i.e.*, the rote-learning situation or the

conditioning situation), rational equations which fit considerable proportions of the data have been developed. Such equations have been formulated by Thurstone (1930), Hull (1940, 1943, 1950), Estes (1950) and others.[14]

There is a danger, which is not inherent in the use of equations, but which has not always been avoided by past users of them, that one will forget the relativity of curves to their conditions and will regard the equation as a statement of learning as it "really" or "absolutely" is. One must always remember that an equational statement is meaningless in the absence of specification of the conditions under which the learning occurred. In rational equations, of course, this specification of conditions is contained in the constants of the equation themselves.

REFERENCES

Boring, E. G. *A history of experimental psychology.* New York: Appleton-Century, 1929.

Boring, E. G. *A history of experimental psychology.* New York: Appleton-Century, 1950.

Brogden, W. J. Acquisition and extinction of a conditioned avoidance response in dogs. *J. comp. physiol. Psychol.*, 1949, 42, 296-302.

Carr, H. A. *Psychology: a study of mental activity.* New York: Longmans, Green, 1925.

Dollard, J., and Miller, N. E. *Personality and psychotherapy.* New York: McGraw-Hill, 1950.

Ebbinghaus, H. *Über das Gedächtnis: Untersuchungen zur experimentellen Psychologie.* Leipzig: Duncker and Humblot, 1885. (Trans. as *Memory: a contribution to experimental psychology* by H. A. Ruger and C. E. Bussenius. New York: Teachers College, Columbia Univ., 1913.)

Estes, W. K. Toward a statistical theory of learning. *Psychol. Rev.*, 1950, 57, 94-107.

Glaze, J. A. The association value of non-sense syllables. *J. genet. Psychol.*, 1928, 35, 255-267.

Haagen, C. H. Synonymity, vividness, familiarity, and association value ratings of 400 pairs of common adjectives. *J. Psychol.*, 1949, 27, 453-463.

Hall, V. P. Interaction of list and item difficulty in rote-serial learning. Unpubl. M. A. Thesis, Univ. Illinois, 1950.

Hilgard, E. R. A summary and evaluation of alternative procedures for the construction of Vincent curves. *Psychol. Bull.*, 1938, 35, 282-297.

[14] Thurstone's (1919) monograph gave impetus to much of the later work on equations of learning curves. In connection with it, cf. the papers by Meyer and Eppright (1923) and by Valentine (1930). Papers by Woodrow (1940, 1942) contain a discussion of learning curve equations and references to other papers on the subject.

Hilgard, E. R. Methods and procedures in the study of learning. *In* Stevens, S. S. (Ed.), *Handbook of experimental psychology*. New York: Wiley, 1951.

Hollingworth, H. L. *Psychology: its facts and principles*. New York: Appleton, 1928.

Hull, C. L. The meaningfulness of 320 selected nonsense syllables. *Amer. J. Psychol.*, 1933, 45, 730-734.

Hull, C. L., *et al. Mathematico-deductive theory of rote learning: a study in scientific methodology*. New Haven: Yale Univ. Press, 1940.

Hull, C. L. *Principles of behavior; an introduction to behavior theory*. New York: Appleton-Century, 1943.

Hull, C. L. Behavior postulates and corollaries—1949. *Psychol. Rev.*, 1950, 57, 173-180.

Hunter, W. S., and Yarbrough, J. U. The interference of auditory habits in the white rat. *J. Anim. Behav.*, 1917, 7, 49-65.

Husband, R. W. Human learning on a four-section, elevated, finger maze. *J. gen. Psychol.*, 1928, 1, 15-28.

Kjerstad, C. L. The form of the learning curves for memory. *Psychol. Monogr.*, 1919, 26, No. 116.

Lewis, D., and Shephard, A. H. Devices for studying associative interference in psychomotor performance. I. The modified Mashburn apparatus. *J. Psychol.*, 1950, 29, 35-46.

McGeoch, J. A. The vertical dimensions of mind. *Psychol. Rev.*, 1936, 43, 107-129.

McNemar, Q. *Psychological statistics*. New York: Wiley, 1949.

Melton, A. W. The methodology of experimental studies of human learning and retention. I. The functions of a methodology and the available criteria for evaluating different experimental methods. *Psychol. Bull.*, 1936, 33, 305-394.

Melton, A. W. The end-spurt in memorization curves as an artifact of the averaging of individual curves. *Psychol. Monogr.*, 1936, 47, No. 212, 119-134.

Melton, A. W., and Safier, D. E. Meaningful similarity of pairs of two-syllable adjectives. From *Materials for use in experimental studies of the learning and retention of verbal habits*. Univ. Missouri, 1936. (Mimeo.)

Meyer, M. F., and Eppright, F. C. The equation of the learning function. *Amer. J. Psychol.*, 1923, 34, 203-222.

Miller, N. E., and Dollard, J. *Social learning and imitation*. New Haven: Yale Univ. Press., 1941.

Pavlov, I. P. *Conditioned reflexes: an investigation of the physiological activity of the cerebral cortex*. (Trans. and ed. by G. V. Anrep.) London: Oxford Univ. Press, 1927.

Shephard, A. H., and Lewis, D. Devices for studying associative interference in psychomotor performance. II. The modified two-hand coordinator. *J. Psychol.*, 1950, 29, 53-66.

Thorndike, E. L. Animal intelligence: an experimental study of associative processes in animals. *Psychol. Monogr.*, 1898, 2, No. 8.

Thurstone, L. L. The learning curve equation. *Psychol. Monogr.*, 1919, 26, No. 114.

Thurstone, L. L. The learning function. *J. gen. Psychol.*, 1930, 3, 469-491.

Valentine, W. L. A study of learning curves. II. The relationship between the hyperbola and the arc cotangent function. *J. gen. Psychol.*, 1930, 4, 359-362.

Vincent, S. B. The function of the vibrissæ in the behavior of the white rat. *Behav. Monogr.*, 1912, 1, No. 5.

Warden, C. J. The relative economy of various modes of attack in the mastery of a stylus maze. *J. exp. Psychol.*, 1924, 7, 243-275.

Warren, H. C. *A history of the association psychology*. New York: Scribner's, 1921.

Witmer, L. R. The association value of three-place consonant syllables. *J. genet. Psychol.*, 1935, 47, 337-359.

Woodrow, H. Interrelations of measures of learning. *J. Psychol.*, 1940, 10, 49-73.

Woodrow, H. The problem of general quantitative laws in psychology. *Psychol. Bull.*, 1942, 39, 1-27.

III

SOME THEORETICAL CONSIDERATIONS

INTRODUCTION

As FACTUAL knowledge concerning the learning process has advanced, theories of the learning process have also been developed. These theories have, in some instances, represented an attempt to consolidate existing knowledge, but they have also served to suggest and to direct a very great many research projects. Fact and theory cannot, therefore, be clearly untangled since the development of each has been dependent upon the status of the other, and the psychology of learning cannot be understood from the standpoint of either, alone. This book deals largely with factual information. The problems of theory, however, so often dominate the facts that we must review, however briefly, some of these problems before going on to the material with which this work is more fundamentally concerned.

It would be presumptuous to attempt to summarize all, or even several, of the theories of learning in a single chapter.[1] Instead, we shall attempt to illustrate the role theory should play in our science, and shall give a brief discussion of some persistent theoretical problems in the psychology of learning, together with some representative solutions to these problems.

THE NATURE OF SCIENTIFIC THEORY

In order to understand the role of theory in science, it is necessary, first, to understand the nature of science itself. A scientific statement of fact is a statement concerning the nature of the

[1] For a detailed consideration of learning theories, Hilgard (1948) may be recommended. Spence's articles in Moss (1942) and Stevens, *et al* (1951) are of especial importance. Better yet, the serious student should consult the original sources on which these reviews and summaries are based.

relationship between two or more variables or events which occur in the natural world. In its simplest form, such a statement merely notes that one event follows another event in a regular fashion. Thus, we may note that if an infant is pricked with a pin (event 1) the crying of the infant (event 2) regularly follows. The statement of this relationship may be refined, somewhat, if we care to quantify the two events and to note that the infant tends to cry more vigorously or for a longer period of time following an intense pinprick than following a weak one. Fundamentally, however, the fact lies in the two (or more) events and the nature of the dependency between them. The objective of the scientist is to determine such relationships between operationally defined variables.[2] In practice, of course, the scientist attempts, not only to establish isolated dependencies, but to incorporate these into larger systems of relationships which will allow him to predict empirical findings over a wide range of experimental conditions.

Theory, in its simplest form, is nothing more than a guess at the relationship which may exist between two operationally defined variables. Such a guess may be highly specific, or it may merely state the relationship in general terms. Again, in practice, the scientist is generally interested in guessing at a greater number of relationships than at a single one. He attempts to piece together his available facts into some larger framework of relationships, guessing where necessary, to fill in the gaps in this system.

Theories may be classified in various ways. One of the most apparent methods is to classify theories in terms of the specificity of the guesswork done. Physical science furnishes many examples of extremely precise and specific theoretical work, of hypotheses which predict experimental findings to within a thousandth part of a second or of a millimeter. Perhaps psychological theory will never become this specific in its theorizing, but even a cursory examination of contemporary psychological theories will reveal astonishing

[2] For an extended discussion of the logic of science, the student should consult such additional sources as Bergmann (1943), Bergmann and Spence (1941), Bridgman (1927), Cohen and Nagel (1934), Pratt (1939), Spence (1944, 1948), and the symposium on operationism which appeared in the *Psychological Review* in 1945.

differences in the degree of specificity of the theoretical work. For example, psychoanalytic theory, if such it may be called, involves the hypothesizing of extremely vague relationships, oftentimes between undefined variables. In the field of learning, the theories of Thorndike (1931, 1932), Guthrie (1935), and Tolman (1932) hypothesize the variables between which relationships exist, but state these relationships only in very general terms. In a sense, these theories represent an attempt to isolate the important variables of the learning process rather than an attempt to predict learning behavior precisely. On the other hand, the theories of Hull (1940, 1943, 1950) and the promising beginnings made by Estes (1950) represent much more precise theorizing.[3] If anything, the precision of some of this theoretical work goes beyond the precision of present experimental findings.

The specificity of the theory is important in a number of ways. By and large, the more specific of two theories is the better, providing that both stand up equally well under empirical investigation. It is this latter provision, however, which constitutes the major difficulty for a theory which makes highly specific predictions. Any scientific theory stands or falls in terms of empirical validation, and no theory can be retained in the face of a single contradictory fact. The validity of a theory is, thus, an all-or-none affair, and no matter how many facts and relationships agree with a particular theory, a single, well-established contradictory fact causes its downfall, at least until appropriate theoretical modifications can be made. Contradictory facts, themselves, however, require definition, and a small amount of thought will show that there are at least two kinds of contradictory facts which, for want of better terms, we shall call contradictory facts of *kind* and contradictory facts of *approximation.* By the latter, we mean experimental results which approach the theoretical prediction within the range of precision of experimentation and measurement. Contradictory facts of *kind* are experimental

[3] Other relatively precise theoretical work, such as that of Pitts (1943) and Von Förster (1948) may be mentioned in passing. These theories are confined to a more limited area than the theories mentioned above, and have, to the present time, been less influential in determining experimental work.

results which do not approach the theoretical prediction within this range. By increasing laboratory precision and by the repetition of experiments we can change many supposed errors of approximation into contradictory facts of *kind*. Whether or not a theory survives depends upon whether contradictory facts of kind may be established.[4] It is at this point that the advantage (as regards viability) of the vague over the specific theory is found. If theoretical predictions are made in general, non-quantitative terms, many "contradictory facts" may be regarded as errors of approximation rather than as contradictory facts of kind. On the other hand, the precisely quantified theory may come fairly close to predicting experimental results, but may require modification because the divergence between fact and theory is greater than can be accounted for by experimental error.

Theories may also be differentiated in terms of their generality. Some hypotheses are designed to account only for a limited area, a particular combination of experimental facts. Other theories are much more general in scope, taking as their province all of conditioned response learning or all of learning as a whole. In view of the present lack of detailed experimental knowledge, it is to be expected that the more general theories also tend to be the ones which make the less specific predictions. At the present time there is no general, specific theory which encompasses the learning process as a whole.

In summary, then, we may say that the objective of science is to establish relationships between operationally defined variables.[5] In

[4] Obviously we refer to survival of a theory in unchanged form. Survival also depends upon the flexibility of the system, that is, the extent to which minor changes in theory may be made which will reconcile theory with fact. All psychological theories show the effects of such theoretical patchwork, some of them to the extent that they resemble the Ptolemaic theory of the solar system with its complications of cycles and epicycles. Many, much-patched theories of learning, no doubt, are destined also for a similar end.

[5] A popular misconception has it that science is a body of knowledge, and that there are different sciences corresponding to these different fields of information. Actually, science is a set of rules for the definition of fact and these rules, called the scientific method, are basically the same for all "sciences." The different fields of science represent customary and convenient divisions of labor among scientists and no one group of scientists is isolated from other

general, the scientist hopes to determine groups of relationships rather than isolated ones. Theories represent guesses at relationships between operationally defined variables which have not yet been empirically determined. Typically, theories represent a patchwork of previously observed fact and present guesswork. Such theories attempt to predict what will occur under a specified set of experimental conditions, and a particular theoretical system retains its validity only so long as predictions made from it are in essential agreement with empirical fact. Since theories which make vague predictions tend to survive this laboratory ordeal more successfully than theories which make highly specific predictions, we may expect to observe a greater number of vague than specific theories. On the other hand, highly specific, quantitative theories represent the ultimate scientific goal. At the present time learning theories tend to restrict themselves to the explanation of limited ranges of data. The greater this limitation, the more specific the predictions can become. There are, at present, no specific theories of the learning process in general.

SOME PERSISTENT THEORETICAL PROBLEMS

Certain theoretical problems have been persistent sources of dispute among psychologists who study the learning process. Some of the more important of these problems are discussed below. It should not be thought, however, that the topics considered represent an exhaustive analysis of these theoretical problems.

Are There General Laws of Learning?

The problem here is to determine whether or not principles and relationships determined in one learning situation may be validly applied to other learning situations. Can laws of learning derived

scientists. It may also be noted, in this connection, that eventually unification or reduction of the sciences may be expected. Thus, psychology may become explainable in terms of physiology, physiology in terms of chemistry, chemistry in terms of physics, and so on. There is no immediate danger, however, that the student of psychology will find his functions usurped by the physiologist, nor is there any validity to the tiresome complaint that physiological knowledge of learning (or anything else) is the only "true" knowledge concerning this process.

from the study of conditioning be applied to maze learning or trial-and-error learning? Does the learning of autonomically innervated responses proceed in the same fashion as other learning? Can the results of experiments with animals be applied to the learning of human beings? These are, of course, some of the special problems which are contained in the general one. It is possible, for example, that general laws of learning may apply for the learning of human beings, but that these differ from those involved in the learning of other species. Similarly, over a wide range of species, some responses may be learned according to one set of principles, while other responses may be acquired under different laws.

Concerning the problem of differences among species with respect to the basic principles of learning, there is a growing mass of evidence that the basic processes of learning are similar over a wide range of species. This evidence, however, is indirect rather than direct, and consists mainly of the fact that no significant variations in this respect have been noted although thousands of animal and human learning experiments have been performed. This is not to say that human and animal learning are identical, but only that the former can probably be regarded as a complex variety of the latter.[6] The complexity of the learning process in human beings far exceeds its complexity in other species. Nevertheless, that the distributions of learning scores for men and animals overlap in various ways has been shown by Gardner (1945) and others. In a recent article, Seward (1948) defends in detail the position that the basic behavioral mechanisms as between species are homologous. Although it will never be possible to prove the continuity of basic learning principles between species, lack of negative evidence should incline us to the belief that this continuity exists. Such a belief is in keeping with the principle of parsimony and serves to emphasize the practical importance of learning experimentation with species other than man.

The problem of the generality of the laws of learning between various learning situations is, perhaps, more significant. Certainly

[6] For the opposed view, that animal learning and human learning are fundamentally dissimilar phenomena, see Allport (1947).

the evidence in this instance is much less clear. Regardless of the basic laws of learning which may be assumed, it is important to know whether these laws are really basic, or whether they apply only to a limited range of learning situations. Two approaches to this problem present themselves. One may begin with some complex variety of learning which one regards as basic and attempt to derive simple forms of learning as "special cases" which follow the same general laws. Conversely, one may start with simple forms of learning and attempt to show how complex learning may be derived as a function of the summation and interaction of less complex habits and processes. In either case, the basic principles remain relatively unchanged. As an example of the first type of approach, Thorndike's (1931) work will serve. Taking trial-and-error learning as basic, Thorndike regarded conditioned response learning as a special and less important case. On the other hand, the early theoretical papers of Hull (1930a, 1930b, 1931, 1932, 1937, 1938) represent an attempt to explain more complex forms of learning in terms of conditioning principles. A different approach is represented by those who hold that there are two (or more) basic types of learning. Skinner (1935, 1938) in his early work distinguishes between classical and instrumental conditioning in such a way as to imply that different laws of learning apply between them. More recently, Mowrer (1947) has assumed two basic types of learning. According to this view, responses based on autonomic innervation (emotional responses) are learned according to the principle of temporal contiguity, while skeletal responses are supposedly learned according to the principle of reinforcement or effect. There are not enough relevant data at the present time to afford a basis for an evaluation of these proposals.

Recently Tolman (1949) has proposed that six types of learning exist. These six types (cathexes, equivalence beliefs, field expectancies, field-cognition modes, drive discriminations, and motor patterns) are assumed to follow different principles of acquisition and retention. Tolman's classification does not follow any apparent physiological pattern although the types of learning situations which he describes may be clearly differentiated. The virtue of

Tolman's plausible hypothesis is that it provides potential explanation for a wide range of learning phenomena. This wide explanatory coverage, however, is to be expected in any case where six groups of principles are assumed. Certainly, the evidence in support of such a classification is far from conclusive, and, in view of the relative lack of parsimony involved, it would seem wise to reject this view for the time being and until a considerably greater body of experimental data forces its acceptance.

No current view of the learning process represents the field of learning as entirely chaotic—that is—with different laws applying to every distinguishable learning situation. One or several types of learning may be assumed, but always these types are far less numerous than the number of learning situations they are designed to explain. Indeed, were it otherwise, no general psychology of learning could exist and the study of learning would consist of a purely empirical study of each type and variation of learning situation for purely practical reasons. Considering the wide diversity of learning situations and the number of different responses which may be learned, one is struck more by the generality of learning principles than by their diversity. Although no answer may be given to the question, how many types of learning are there, at the present time it is the writer's belief that a single set of principles will suffice. Certainly this is the parsimonious view to take until such time as the necessity for multiple types is conclusively demonstrated.

A word should be said concerning some of the special problems raised by the use of highly complex situations in the study of human learning. The factors of language and meaning complicate the study of learning in many ways, and a host of special concepts are necessary to deal with such complex learning situations. All that has been said concerning the generality of the laws of learning should not be taken to mean that, with complex learning situations, additional principles, not present in simple learning situations, may not be introduced. All that is implied is that these new concepts are complications of or additions to the ones used to explain simple learning rather than substitutes for them. In many cases, and perhaps eventually in all, it will prove to be possible to derive these additional

concepts from the more basic ones, and this ability to derive the complex from the simple will undoubtedly increase with additional knowledge.

What Is Associated

The fact of learning implies the establishment of functional relationships of some sort so that a particular situation comes to elicit (at least under proper conditions) some new variety of response. The establishment of such functional relationships may be termed association, but the nature of these associations and the events which become associated has been a source of considerable theoretical dispute. On the level of experimental operations, situation and behavior become linked together through the learning process so that the presentation of the situation is followed by the response in question with a relatively high degree of probability. Some theorists, sometimes referred to as stimulus-response psychologists, have formulated their theories close to the level of these experimental operations and conceive of habits as functional relationships established between stimuli, on the one hand, and responses, on the other. Thorndike, Hull, and Guthrie may be considered as being representatives of this group. Other theorists have attempted to go beyond this level and to describe learning in terms of associated ideas or cognitions, the resulting habits being formed and existing independently of any response made by the organism. Such theories emphasize the afferent functions and tend to conceive of learning in terms of perceptual reorganization or in terms of associated cognitions, *i.e.*, that this stimulus will be followed by that second stimulus. In some instances, classical conditioning for example, this cognition is sufficient, although in cases of instrumental conditioning or maze learning, the cognition that one event leads to another is accompanied by a second cognition concerning the behavior route which must be followed if this sequence is to occur. The student will recognize, in this brief statement, the general position taken by Tolman. The Gestalt school in general tends to support this type of theory, although many Gestalt psychologists tend to place far more emphasis upon the intrinsic organizing characteristics of the indi-

vidual than does Tolman. The earlier Gestalt writings of Koffka (1935) and Köhler (1929) emphasize the dynamic interactions of the entire perceptual field rather than simple bonds or associations existing between two or more items in this field.[7]

A question at once arises whether there is any difference between these two conceptions, however different their form may appear to be. The crucial question which must be asked is, can any operations be found which will differentiate between these two views? In other words, do these two general theoretical positions lead to different deductions which may be investigated experimentally? At the present state of our knowledge, it does not appear to the writers that any such differentiation is possible. It may well be that this is only a pseudo-problem arising from a differential use of words to describe the same operations. Certainly, any crucial test would seem to involve the occurrence of conditioning in an animal wherein the sensory apparatus was intact but in which the response apparatus was centrally deactivated during the acquisition process. It is unlikely that these operations will become performable in the near future, and it appears unlikely that this problem can ever be solved at the behavioral, as contrasted with the neurophysiological, level.

Until neurophysiological answers become available, however, this discussion is likely to continue. Such discussion must necessarily be confined to an argument about words, *i.e.*, "our words are better than your words," and is evidently futile. In terms of research this discussion is likely to have little effect. Both groups will continue to devise situations and to measure responses. Between these two classes of events, constructs translating the one to the other will continue to be developed, and, by whatever name, must inevitably possess the same formal mathematical properties if equivalent predictions are to be made.

[7] An interesting variation of the concept that learning represents the reorganization of percepts may be found in Woodworth's (1947) paper.

The Use of Intervening Variables

Such constructs relating stimulus and response variables are often referred to as intervening variables. Such variables stand for the various mathematical manipulations to which the stimulus variables are subjected if the response variables are to be predicted. They include the mathematical interactions of the stimulus quantities as well as certain constants introduced to account for inherent and/or temporary states of the organism. Although the term, intervening variable, was originally introduced by Tolman, Hull's (1943, 1950) system offers one of the clearest examples of its use.

In practice, Hull (1943) defines certain intervening quantities in terms of various stimulus or experimental variables. The intervening variable, habit strength $_{S}H_{R}$, for example, is defined in terms of four experimental variables (number of reinforcements, kind and amount of reinforcement, delay in reinforcement, and degree of temporal contiguity between stimulus and response) as well as certain constants which are empirically determined. Habit strength, thus defined, is further modified by various other experimental variables. Habit strength, stimulus similarity, motivation, work performed, the distribution of that work in time, etc., define another intervening variable called effective reaction potential $_{S}\bar{E}_{R}$. This in turn, is modified by an assumed property of the organism which causes behavioral oscillation to determine momentary effective reaction potential, $_{S}\dot{\bar{E}}_{R}$. This last intervening variable is then related to various response measures which serve as experimental indices of learning.

Intervening variables are useful in that they allow a less cumbersome mathematical system than would be obtained if all of the stimulus variables and the constants relating to the learner entered the formulation simultaneously. On the other hand, Hull's formulation could be written in a single equation for the prediction of any one response measure.

In connection with this discussion, it should be emphasized that the intervening variables used in such a system are mathematically defined in terms of experimental operations or postulated character-

istics of the organism. The names given to some of the variables may cause misunderstanding and confusion unless it is understood that the names mean nothing more than the mathematical definitions.

Contiguity and Reinforcement

One of the central issues dividing learning theories concerns contiguity and reinforcement (law of effect). For some theorists, temporal contiguity of situation and response (Guthrie) or of two stimulus-cognitions (Tolman) is sufficient to produce learning. Other theorists, such as Thorndike and Hull, have emphasized the necessity for subsequent reinforcement or reward.[8] Before examining this issue, it is necessary to make certain distinctions. There is little dispute concerning the fact that rewards, non-rewards, and punishments (these will be more clearly defined later) have an effect upon learned performances. The dispute arises over their nature and their mode of operation. Guthrie (1935), for example, does not deny the efficacy of rewards in producing learning. He does insist, however, that these rewards influence learning in a particular way and that the manner of their operation may be deduced from more elementary concepts. Under this theory it is held that temporal contiguity between a stimulus complex and a response is the basic condition of learning. Every time a response occurs in strict contiguity with such a constellation of stimuli the two become associated completely, in an all-or-none fashion. If the stimulus pattern remains unchanged and a second response is caused to take place, complete unlearning of the first *S-R* connection and complete learning of the second *S-R* connection occurs. Reward is held to be efficacious in producing learning because it changes the pattern of stimulation, both external and internal. Thus, the rewarded response is the last response to occur under the stimulus pattern which immediately precedes problem solution, the *S-R* connection involved is not "unlearned," and the response tends to

[8] Although he was one of the originators of the law of effect, Thorndike did not altogether discount the possibility that small amounts of learning might occur as a function of temporal contiguity of stimulus and response, alone.

be repeated when next this stimulus pattern is presented. In this manner, Guthrie accounts for the observable efficacy of rewards in terms of the principle of contiguity. For Tolman, similarly, reward or reinforcement is held to be of little importance. For Tolman, learning consists in the formation of cognitions or hypotheses concerning "what leads to what" and rewards and/or punishments merely serve the purpose of giving confirmation to these already learned cognitions. In this case, the principle of reinforcement may be regarded as a principle of *performance* rather than a principle of *learning*.

Thorndike and Hull, on the other hand, regard the principle of reward as irreducible to more elementary concepts. They are not in complete agreement, however, as to the nature of the reinforcing process. Thorndike (1931) defined reward in terms of behavior, thus:

> "By a satisfying state of affairs is meant one which the animal does nothing to avoid, often doing things which maintain or renew it." [9]

In his *Fundamentals of Learning* (1932), Thorndike assumed the existence of a mediating reinforcing state known as the confirming reaction. This, presumably, occurs in the presence of a satisfying state of affairs and tends to strengthen stimulus-response connections operating at that time.

Hull's (1943) analysis of reward identifies two main types, primary and secondary. Primary reinforcement is associated with drive reduction while secondary reinforcement is acquired through a learning process. According to this view, any stimulus which is consistently associated with a reinforcing state of affairs acquires

[9] This definition was accompanied by a similar definition of an annoying state of affairs. At this time, Thorndike assumed that satisfying aftereffects caused a strengthening of *S-R* connections while annoying aftereffects weakened them. This dual law he later abandoned (1932). It should also be noted that this definition, which merely classifies situations as satisfiers, and non-satisfiers, is not circular with respect to learning as has often been alleged. It may also still the criticism that the law of *effect* is really a law of *affect*, although, in this connection, it is interesting to note the continuity between this definition and Carr's (1925) and Peters' (1935) definitions of pleasantness under the judgmental theory of feeling.

secondary reinforcing properties, and may subsequently operate to reinforce *S-R* connections in the absence of drive reduction. Thus, the chimpanzees in Wolfe's (1936) experiment, having first learned to use tokens to secure food, subsequently learned other problems to secure these tokens. Similarly, the rats in Bugelski's (1938) experiment associated an auditory stimulus with food reward. This auditory stimulus was later shown, in the absence of food reward, to possess reinforcing properties. According to Hull's theory, the occurrence of either type of reinforcement immediately following a stimulus-response sequence is a necessary condition for the learning of that sequence.

A few words concerning the nature of secondary reinforcement should be introduced here, in view of the fact that this concept is one of the most powerful explanatory mechanisms in the repertoire of the reinforcement theorists (cf. Melton, 1950). Not only does this concept provide potential explanations, in reinforcement terms, for learning phenomena, such as latent learning, which lie beyond the explanatory reach of the concept of primary reinforcement, but it also serves to make reinforcement theory more plausible in certain other respects. For example, the series of studies on the delay of reward from Watson (1917) to Grice (1947) have shown that, as the number of secondary reinforcing stimuli are reduced, immediacy of reward becomes increasingly essential for efficient learning. In the absence of secondary reinforcing stimuli, the type of goal gradient function assumed in the early work of Hull (1932) has become untenable. In fact, Grice's results stress the need for such immediacy of reward that ordinary feeding becomes an improbable source of reinforcement for considerable amounts of learning, since the time between the performance of the correct response and the actual drive reduction caused by food-in-the-stomach-and-blood-stream is too long for efficient learning. Secondary reinforcement, of course, provides an answer to this problem. Since sight, odor, etc., of the food, and especially the sensations of food-in-the-mouth, have so consistently been associated with drive reduction, food itself has become a powerful secondary reinforcing stimulus (regardless of whether or not it is ingested). It may be true that in many experi-

ments which employ the feeding of hungry animals, presence of food rather than hunger reduction has served as the principal source of reinforcement. Regarding the problem of the goal gradient, Spence (1947) has proposed that this be considered as a gradient of diminishing amounts of secondary reinforcement rather than as a temporal gradient of primary reinforcement. He suggests, furthermore, a mechanism by means of which this gradient could be established.

There are several possible interpretations of secondary reinforcement. If one regards the reinforcement process in terms of a Thorndikian confirming reaction, secondary reinforcement might be considered as an example of the conditioning or learning of this reaction so that it becomes evokable by previously neutral cue-stimuli. This is, of course, essentially the view which has been presented in the preceding paragraphs. On the other hand, secondary reinforcement may be considered as consisting of the reduction of a secondary or learned drive state (cf. Miller and Dollard, 1941). Some types of learned responses result in a considerable amount of response-based stimulation. Learned emotional responses, for example, exhibit this characteristic. Just as primary reinforcement may be taken to represent a reduction in a primary drive state (hunger, thirst, pain, etc.), so secondary reinforcement may be interpreted as a reduction of a learned drive state. At the present time, however, it is probably best to retain, as separate concepts, secondary reinforcement and secondary drive reduction, both to be considered as reinforcing states of affairs.[10]

Crucial evidence in support of or contrary to the principle of reinforcement in its various forms does not exist. In some cases, it is probable that no operations for the performance of a crucial experiment exist. Thus, the principle of reinforcement stated as drive reduction probably cannot be differentiated experimentally from Guthrie's deduction of the same empirical relationships from the law

[10] Eventually, of course, experimental data may require the consolidation of these concepts. Crucial evidence indicating their separateness could be obtained by demonstrating that stimuli associated with secondary drive reduction could acquire secondary reinforcing characteristics.

of contiguity. The studies of latent learning, initiated by Tolman and his associates and carried on later by Spence and his followers, offer conflicting evidence, both experimentally and theoretically, concerning this issue. The conditions under which latent learning may occur are in some doubt, for while Blodgett (1929), Tolman and Honzig (1930), Buxton (1940), Meehl and MacCorquodale (1948) and Seward (1949) have obtained evidence supporting the existence of latent learning under some conditions, the results of Reynolds (1945), Spence and Lippitt (1946), Kendler (1947), Grice (1948), and Kendler and Mencher (1948) have not. As Hilgard (1948) and Melton (1950) have observed, latent learning may be obtainable only under conditions of relative satiety during original training. In view of the conflicting evidence regarding latent learning, and in view of the possibility that secondary reinforcement plays an important role in these latent learning experiments, this type of evidence probably cannot be regarded as crucial at the present time.

Relatively powerful evidence favoring the necessity for a principle of reinforcement is found in the work of Loucks (1935) who failed to condition leg flexion elicited by faradic stimulation of the motor cortex (in place of an unconditioned stimulus). When food reward followed the flexion response (thus elicited), learning occurred, but without this additional source of reinforcement no conditioning was obtained. Similarly, the study of Loucks and Gantt (1937) demonstrated that conditioning of leg flexion elicited by electrical stimulation of the spinal cord would not occur unless pain fibers (with the attendant opportunity for reinforcement) were stimulated. A considerably greater number of studies employing this promising technique should be brought to bear upon the problem of reinforcement.

In the studies of human learning, rewards and punishments typically take the form of symbols ("right" and "wrong"), or in terms of some other indication of the learner's progress. Reinforcement of this type is often referred to as "knowledge of results," and may be interpreted in terms of secondary reinforcement and secondary drive reduction. The extensive experiments of Thorndike and his

followers have demonstrated the efficacy of this type of reinforcement. It may be shown that such knowledge of results is a more powerful factor than relatively mild nociceptive stimulation. Thus, Tolman, Hall, and Bretnall (1932) found that giving electric shock for correct responses caused learning of those responses (by human adults). The subjects, in this case, of course, understood that shock would serve this informative function.

The Principle of Frequency

The law of frequency is one of the oldest and most widely accepted principles of learning. It is also one of the least specific. In order to give meaning to this law it is necessary to answer two basic questions: Is frequency necessary, and if so, what is it that is frequent?

Frequency is, of course, one of the experimental variables of learning experiments and is typically the variable against which progress is plotted in drawing learning curves. The usual finding, of course, is that learning progresses with frequency. Nevertheless, the role of frequency in producing this improvement is subject to a number of theoretical interpretations. On the one hand, learning may be regarded as a gradual process, in which frequency is the bearer of the incremental effects of practice. On the other hand, learning may be regarded as a sudden process. In this case, frequency serves as a bearer for the conditions under which sudden learning occurs. A number of presentations may be required, for example, to make these conditions combine in a manner favorable to the occurrence of learning.

If learning is regarded as a gradual process, the role of frequency is obvious. From one trial to the next, increments in habit strength summate so that, other things being equal, the most frequently practiced habit becomes the strongest. The associationistic school, generally, the Pavlovians, the Thorndikians, the Watsonians and the Hullians are representatives of this point of view.

For other theorists, a frequency of greater than one is regarded as being unnecessary (at least in theory) and the apparently gradual

nature of learning is explained as being an artifact of the methods of experimentation and measurement. The Gestalt school, generally, and, surprisingly, Guthrie, take this point of view. The de-emphasis which the Gestalt school has placed upon the principle of frequency undoubtedly stems from the tendency to use insight as an explanatory principle of learning. Since insight is, by definition, a sudden process, frequency becomes a relatively less important principle. Under these circumstances, frequency serves the function of providing the conditions which favor insight. These may be repeated frequently, but insight itself is achieved suddenly.[11] According to this point of view, situations may be arranged which so favor the occurrence of insight that it may occur on the first presentation of the situation to the subject. The apparently gradual form of the relationship between improvement and frequency is regarded as resulting from two artifacts: the massing of data and the failure to measure partial insights. If, in a group of subjects, each subject achieves insight at a different point in practice, the learning curve becomes the summation of such all-or-none improvements over the entire group. With a sufficiently large number of subjects, the learning curve will be relatively smooth and, if it be assumed that time to achieve insight is normally distributed, will approach the form of the normal ogive. Negatively accelerated curves may be produced if the problem is relatively easy so that the bulk of the subjects achieve insight early in practice. The fact that individual as well as group learning curves show gradual improvement may be explained by the occurrence of "partial insights."[12] This explanation is a rather

[11] A special aspect of this point of view is the non-continuity theory of discrimination learning proposed by Lashley (1929) and elaborated by Krechevsky (1932, 1938). According to this viewpoint, learning is a discontinuous process wherein the trials which have preceded the attainment of insight (hypotheses) supposedly have no effect upon the nature of the post-insight performance. Opposed to this is the continuity viewpoint which has been vigorously (and, in the opinion of the writers, successfully) defended by Spence (1936, 1940, 1945) and by Ehrenfreund (1947).

[12] The gradual learning of individuals may also be accounted for in terms of a kind of dual learning process. It may be held that the experimental conditions deprive the learner of an opportunity to have insight and that, consequently, learning was not insightful, i.e., occurred under conditions wherein frequency played its usual role. Such learning situations, however, are held to be artificial and atypical of learning in the natural state.

weak one, however, especially when it is applied to relatively homogeneous learning tasks.

The status of the concept of insight in the psychology of learning need be mentioned only briefly. As an empirical phenomenon, insight has been defined in a number of ways, the most usual definitions being in terms of sudden solution of a problem situation and relatively high retention value of such problem solutions. Defined in this way, there is little doubt that insight occurs. Whether this concept can now be used to explain the phenomena which define it is another matter entirely. In view of this circularity, the value of the concept of insight for explanatory purposes is highly questionable. Particularly is this true in view of the probability that the empirical phenomenon of insight may be itself explained in terms of transfer of training (cf. Harlow, 1949).

Guthrie's explanation of the role of frequency is highly ingenious. According to Guthrie (1935), learning is a sudden, automatic, and all-or-none process. Whenever a stimulus complex and a response occur together in strict temporal contiguity, these two events are assumed to become completely associated. This association, furthermore, is assumed to be permanent unless the stimuli in question become associated to another response. The gradual nature of improvement is explained in terms of the inconstancy of the stimulus situation. It is undeniably impossible to reproduce stimulus situations exactly. According to Guthrie, frequency serves the role of conditioning the response to a greater and greater number of stimuli, so that, after a number of trials, the response has been conditioned to a considerable range of stimulus variations and, hence, is more likely to be elicited on a given trial by the particular stimuli operating on that trial. In a sense, then, frequency acts to determine the number of habits learned rather than the strength of a single habit, and the gradual improvement which we observe becomes an artifact of the inability of our measurements to distinguish between these two events.

The question, what is frequent, is more difficult to answer. From the time of Aristotle through the associationistic treatment of learning, frequency has usually referred to the frequency of contiguous

experiences, movements, ideas, or stimuli and responses. It is true that most associationists recognized that some other factors than contiguity (recency, similarity, and vividness, for example) played a role in learning. The typical treatment, however, made temporal contiguity the reference point for the operation of all of these conditions as well as for the operation of frequency. This may be seen, even at the present time, in experiments which demonstrate that frequency, alone, has little influence. Such experiments are usually designed to show that contiguity without reinforcement is not productive of learning, but the tendency to identify frequency with contiguity, alone, is instructive. Quite obviously, frequency must refer to the conditions of the learning process. If these are multiple, then frequency should be applied to them together. In view of the current disagreement concerning the nature of the necessary and sufficient conditions of learning, the answer to this question will vary from one theory to another.

Tolman's position, for example, does not differ greatly from that of the British associationists. For Tolman, frequency refers to the temporal contiguity of experiences. For Guthrie, as we have seen, frequency is an unnecessary fundamental concept. As far as learning is concerned, a frequency of one is sufficient, and that frequency of one refers to a contiguous occurrence of a stimulus and a response. For Guthrie, frequency is a law of performance rather than a law of learning.

The reinforcement theorists, however, face a more difficult situation if they decide that both temporal contiguity and the principle of reinforcement must be included. Miller and Dollard (1941), for example, speak of four essential conditions of the learning process. These are drive, stimulus, response, and reward. However, the interrelations of these variables causes further increase in the number of conditions. These four events (when occurring in proper order and sequence) constitute a single trial or unit of practice. Frequency refers to the number of such trials or constellations of conditions. In a similar way, Hull (1943) in his earlier formulation considered that habit strength accumulated through frequency of reinforcement to a theoretical maximum value. This maximum value

(and hence, in this system, the size of the increment resulting from each trial) was determined by three factors: kind and amount of reinforcement, degree of temporal contiguity of stimulus and response, and amount of delay of reinforcement. The effect of frequency upon habit strength (and it should be emphasized that the term, habit strength, refers to Hull's intervening variable, ${}_SH_R$) depends, then, upon these three conditions.[13] Hull (1943, 1950) also introduces frequency in connection with the formation of his inhibitory intervening variable, I_R. Thus, frequency of practice has both an incremental and a decremental effect upon performance.[14] This treatment of inhibition, however, will be discussed in the following section of this chapter.

The law of frequency has typically referred to a single condition of learning which was considered to be "sufficient." Usually this condition has been the contiguous occurrence of the associated items. The development of reinforcement theory, however, has forced a broadening of this reference base. Miller and Dollard (1941), for example, consider four conditions and their interrelations as being basic conditions of learning. Frequency refers to the combination of these conditions which may exist in any learning situation. Similarly, Hull (1943) made frequency refer to a group of experimental conditions. It is to be expected that, as knowledge increases, frequency, if necessary at all, will eventually refer to all of the conditions which have regular incremental or decremental effects upon learning.

[13] In his later postulate system, Hull (1950) regards reinforcement as the only condition of habit strength. Other factors relating to the delay of reinforcement, etc., become determiners of performance rather than of habit, as such. It is difficult to agree that this modification has strengthened the predictive power of Hull's system. Under the latter arrangement, of course, frequency refers only to the number of reinforcements.

[14] One unfortunate deduction follows from both the 1943 and the 1950 systems. Since ${}_SH_R$, once it reaches its maximum, is no longer influenced by continued reinforcement, since I_R does not subtract from ${}_SH_R$, and since no provision is made for differential increments in I_R under conditions of reinforcement and non-reinforcement, it follows that, as soon as ${}_SH_R$ reaches its limit, performance should decline as rapidly under conditions of overlearning (continued reinforcement) as under conditions of extinction (non-reinforcement). This situation could be avoided, however, by having I_R subtract from the value of N (number of reinforcements) rather than ${}_SE_R$.

Decremental Factors in Learning

Learning is not entirely an incremental process. During learning, following practice, and under certain other conditions, inhibitory factors are in operation. These serve either to slow the acquisition process or to cause a decrement in the retention of learned performances. On the other hand, decremental factors do not always have an adverse effect upon learned performances. In complex learning situations, they serve to cause variability of response during the presolution period and to produce heightened efficiency of the habitual performance thereafter. Every elimination of an incorrect or inefficient mode of behavior during the course of practice involves the operation of one or more of the decremental factors, although the end result is positive in terms of measured performance.

In conditioned response learning, the phenomenon of experimental extinction is found. This occurs whenever the conditioned stimulus is given for a number of trials without being paired with the unconditioned stimulus. More generally, extinction tends to occur under conditions of non-reinforcement. Pavlov (1927) explained extinction in terms of a theoretical construct known as internal inhibition. This inhibition developed during practice (reinforced or non-reinforced) and dissipated in time. By means of this hypothesis, Pavlov was enabled to explain a good many of the conditioning phenomena, such as the differential effects of massing trials during conditioning and extinction, inhibition of conditioning, spontaneous recovery, and, with a few additional assumptions, phenomena such as disinhibition. For Pavlov, performance was largely determined by the interaction of the factors of excitation (built up during reinforced trials and relatively permanent) and internal inhibition. Opposed to an internal inhibition interpretation is the interference or counter-conditioning theory of extinction proposed by Guthrie (1935) and Wendt (1936). Such a theory holds that extinction is due to the fact that during unreinforced trials, the learner acquires other (and interfering) associations to the conditioned stimulus, thus causing a disappearance of the

originally conditioned response. This hypothesis has the advantage of being continuous with the retroactive inhibition explanation of the forgettng of complex habits. Its disadvantage is that it does not explain conditioned response phenomena nearly as adequately as does the internal inhibition theory.[15] This is not surprising since Pavlov's hypotheses were designed to explain these phenomena. On the other hand, some intervening variable which has the same formal properties as internal inhibition seems to be required if the facts of conditioning and extinction are to be explained. It is, of course, possible to employ such a concept without accepting Pavlov's somewhat doubtful physiological interpretation of internal inhibition.

Hull (1943) proposed a two-factor theory of inhibition. The first of these factors is called reactive inhibition *(I_R)* and possesses many of the same formal properties as Pavlov's concept of internal inhibition. It does have the additional important characteristic of being determined by the amount of work performed by the organism in making the response. Reactive inhibition is supposed to develop during practice (reinforced or unreinforced) and to dissipate with time. Rate of development depends upon the amount of work performed in making the response. The second of Hull's inhibitory factors corresponds more nearly to the counter-conditioning view. This factor ($_SI_R$) is conceived as a habit of not-responding. This habit develops due to the reinforcement received from a decrease in I_R when the organism ceases to respond. As a habit phenomenon, $_SI_R$ is permanent, and, as such, is useful in explaining the effects of successive extinctions and the failure of complete spontaneous recover to occur. I_R and $_SI_R$ summate, in

[15] Although an extensive review of the evidence on this matter would be out of place here, the interference or counter-conditioning theory has difficulty in explaining: (1) the lack of correlation between rate of learning and rate of extinction, (2) the differential effects of massed practice during learning and extinction, (3) the differential drug effects which are obtained during learning and during extinction, (4) the fact of spontaneous recovery, (5) the differential effects of an extra stimulus introduced during conditioning and extinction (external inhibition and disinhibition), and (6) the occurrence of what Pavlov called inhibition of the second order. This last phenomenon requires verification, but, if verified, would constitute strong evidence against a counter-conditioning theory.

Hull's system, to subtract from the reaction potential ($_{S}E_{R}$) produced by habit as multiplied by drive.

In a recent paper, Tolman (1949) has proposed that six types of learning exist. Each of these, presumably, obeys different laws with respect to acquisition. What about negative factors? Tolman avoids this problem in the case of four of these types: in two cases by assuming the decremental effects do not occur, and in the other two, by noting that no information exists. In the case of the learning of field expectancies, however, Tolman appears to subscribe to a modified form of the law of disuse. Weakening as a result of the mere passage of time is held to occur, but this weakening is not merely a fading out process. Instead, the field expectancies become distorted according to the organization of the individual under Gestalt-like laws. Concerning the retention of motor patterns, however, Tolman adopts a counter-conditioning point of view, that is, one motor pattern is forgotten because another incompatible movement pattern is learned.

The forgetting of complex tasks evidently does not follow the relatively simple pattern of experimental extinction. Best evidence, at the present time, would seem to indicate that forgetting is attributable to three sets of conditions: altered stimulus context, altered set to perform, and retroactive inhibition. The relative importance of these three variables depends, of course, upon the nature of the forgetting situation and the task which is forgotten. Furthermore, several of these general conditions of forgetting are, themselves, complex, and require analysis and explanation. For example, retroactive inhibition may be more or less adequately accounted for by a number of hypotheses and may include experimental extinction as one of its conditions (cf. Melton and Irwin, 1940).

Forgetting under the so-called law of disuse is, however, not acceptable to those who are working in the field of retention and forgetting. Despite the many vigorous attacks which have been made upon it for many years, however, (McGeoch, 1932) the principle of disuse is still invoked by some theorists as an explanation of forgetting.

The Relation of Special to General Theory

In the field of learning, many special theories exist. These are limited hypotheses aimed at the explanation of relatively isolated phenomena. Most of these special hypotheses have not been mentioned in this chapter, but will be covered in contexts appropriate to their content. General theories, of course, are those which apply to a greater body of learning phenomena, and, at the upper limit of generality, become systems of psychology encompassing, not only the data of learning, but other facts of psychology as well. Acceptance of special theories is not entirely determined by their adequacy to explain the limited group of phenomena they are intended to encompass. Unless one adopts a completely eclectic point of view, a certain consistency of approach must be observed. Under a certain general systematic approach, one cannot accept certain special hypotheses and retain logical consistency. This is because the concepts implied by the one conflict with those necessary to the other. Furthermore, even two special hypotheses concerning essentially different phenomena may contradict each other. For example, if one adopts Melton and Irwin's (1940) two-factor theory of retroactive inhibition, one cannot logically accept a counter-conditioning theory of experimental extinction. This is because the two-factor theory holds that retroactive inhibition is attributable to negative transfer or interference and also to experimental extinction. One cannot, then, explain extinction in terms of negative transfer or interference, since that assumption would reduce the two-factor theory to a single-factor theory. In this way, many special hypotheses are interrelated and a demonstration of the validity of one hypothesis may force the rejection of some other special theory in a different area.

REFERENCES

Allport, G. W. Scientific models and human morals. *Psychol. Rev.*, 1947, 54, 182-192.

Bergmann, G. Outline of an empiricist philosophy of physics. I and II. *Amer. J. Physics*, 1943, 11, 248-258; 335-342.

Bergmann, G., and Spence, K. W. Operationism and theory in psychology. *Psychol. Rev.*, 1941, 48, 1-14.

Blodgett, H. C. The effect of introduction of reward upon the maze performance of rats. *Univ. Calif. Publ. Psychol.*, 1929, 4, 113-134.
Bridgman, P. W. *The logic of modern physics.* New York: Macmillan, 1927.
Bugelski, R. Extinction with and without sub-goal reinforcement. *J. comp. Psychol.*, 1938, 26, 121-133.
Buxton, C. E. Latent learning and the goal gradient hypothesis. *Contr. Psychol. Theor.*, 1940, 2, No. 2.
Carr, H. A. *Psychology: a study of mental activity.* New York: Longmans, Green, 1925.
Cohen, M. R., and Nagel, E. *An introduction to logic and scientific method.* New York: Harcourt, 1934.
Ehrenfreund, D. An experimental test of the continuity theory of discrimination learning with pattern vision. *J. comp. physiol. Psychol.*, 1948, 41, 408-422.
Estes, W. K. Toward a statistical theory of learning. *Psychol. Rev.*, 1950, 57, 94-107.
Gardner, L. P. The learning of low grade aments. *Amer. J. ment. Def.*, 1945, 50, 59-80.
Grice, G. R. The relation of secondary reinforcement to delayed reward in visual discrimination learning. *J. exp. Psychol.*, 1948, 38, 1-16.
Grice, G. R. An experimental test of the expectation theory of learning. *J. comp. physiol. Psychol.*, 1948, 41, 137-143.
Guthrie, E. R. *The psychology of learning.* New York: Harpers, 1935.
Harlow, H. F. The formation of learning sets. *Psychol. Rev.*, 1949, 56, 51-65.
Hilgard, E. R. *Theories of learning.* New York: Appleton-Century-Crofts, 1948.
Hull, C. L. Simple trial and error learning: a study in psychological theory. *Psychol. Rev.*, 1930a, 37, 241-256.
Hull, C. L. Knowledge and purpose as habit mechanisms. *Psychol. Rev.*, 1930b, 37, 511-525.
Hull, C. L. Goal attraction and directing ideas conceived as habit phenomena. *Psychol. Rev.*, 1931, 38, 487-506.
Hull, C. L. The goal gradient hypothesis and maze learning. *Psychol. Rev.*, 1932, 39, 25-43.
Hull, C. L. Mind, mechanism and adaptive behavior. *Psychol. Rev.*, 1937, 44, 1-32.
Hull, C. L. The goal-gradient hypothesis applied to some "field force" problems in the behavior of young children. *Psychol. Rev.*, 1938, 45, 271-299.
Hull, C. L., *et al. Mathematico-deductive theory of rote learning.* New Haven: Yale Univ. Press, 1940.
Hull, C. L. *Principles of behavior.* New York: Appleton-Century, 1943.
Hull, C. L. Behavior postulates and corollaries—1949. *Psychol. Rev.*, 1950, 57, 173-180.
Kendler, H. H. An investigation of latent learning in a T-maze. *J. comp. physiol. Psychol.*, 1947, 40, 265-270.
Kendler, H. H., and Mencher, H. C. The ability of rats to learn the location of food when motivated by thirst—an experimental reply to Leeper. *J. exp. Psychol.*, 1948, 38, 82-88.
Koffka, K. *The principles of Gestalt psychology.* New York: Harcourt, 1935.
Köhler, W. *Gestalt psychology.* New York: Liveright, 1929.
Krechevsky, I. "Hypotheses" in rats. *Psychol. Rev.*, 1932, 39, 516-532.
Krechevsky, I. A study of the continuity of the problem-solving process. *Psychol. Rev.*, 1938, 45, 107-133.

Lashley, K. S. *Brain mechanisms and intelligence.* Chicago: Univ. Chicago Press, 1929.

Loucks, R. B. The experimental delimitation of neural structures essential for learning: the attempt to condition striped muscle responses with faradization of the sigmoid gyri. *J. Psychol.*, 1935, 1, 5-44.

Loucks, R. B., and Gantt, W. H. The conditioning of striped muscle responses based upon faradic stimulation of dorsal roots and dorsal columns of the spinal cord. *J. comp. Psychol.*, 1938, 25, 415-426.

McGeoch, J. A. Forgetting and the law of disuse. *Psychol. Rev.*, 1932, 39, 352-370.

Meehl, P. E., and MacCorquodale, K. A further study of latent learning in the T-maze. *J. comp. physiol. Psychol.*, 1948, 41, 372-396.

Melton, A. W. Learning. *Ann. Rev. Psychol.*, 1950, 9-30.

Melton, A. W., and Irwin, J. McQ. The influence of degree of interpolated learning on retroactive inhibition and the overt transfer of specific responses. *Amer. J. Psychol.*, 1940, 53, 173-203.

Miller, N. E., and Dollard, J. *Social learning and imitation.* New Haven: Yale Univ. Press, 1941.

Moss, F. A. (Ed.). *Comparative psychology (rev. ed.).* New York: Prentice-Hall, 1942. (Chapter 12).

Mowrer, O. H. On the dual nature of learning—a reinterpretation of "conditioning" and "problem-solving." *Harvard Educ. Rev.*, 1947, 17, 102-148.

Pavlov, I. P. *Conditioned reflexes.* (Trans. by G. V. Anrep). Oxford Univ. Press, 1927.

Peters, H. N. The judgmental theory of pleasantness and unpleasantness. *Psychol. Rev.*, 1935, 42, 354-386.

Pitts, W. A general theory of learning and conditioning. I and II. *Psychometrika*, 1943, 8, 1-18; 131-140.

Pratt, C. C. *The logic of modern psychology.* New York: Macmillan, 1939.

Psychological Review. Symposium on operationism. *Psychol. Rev.*, 1945, 52, 241-294.

Reynolds, B. A repetition of the Blodgett experiment on "latent learning." *J. exp. Psychol.*, 1945, 35, 504-516.

Seward, J. P. The sign of a symbol: a reply to Professor Allport. *Psychol. Rev.*, 1948, 55, 277-296.

Seward, J. P. An experimental analysis of latent learning. *J. exp. Psychol.*, 1949, 39, 177-186.

Skinner, B. F. Two types of conditioned reflex and a pseudo-type. *J. gen. Psychol.*, 1935, 12, 66-77.

Skinner, B. F. *The behavior of organisms.* New York: Appleton-Century, 1938.

Spence, K. W. The nature of discrimination learning in animals. *Psychol. Rev.*, 1936, 43, 427-449.

Spence, K. W. Continuous versus non-continuous interpretations of discrimination learning. *Psychol. Rev.*, 1940, 47, 271-288.

Spence, K. W. The nature of theory construction in contemporary psychology. *Psychol. Rev.*, 1944, 51, 47-68.

Spence, K. W. An experimental test of the continuity and non-continuity theories of discrimination learning. *J. exp. Psychol.*, 1945, 35, 253-266.

Spence, K. W. The role of secondary reinforcement in delayed reward learning. *Psychol. Rev.*, 1947, 54, 1-8.

Spence, K. W. The postulates and methods of behaviorism. *Psychol. Rev.*, 1948, 55, 67-78.

Spence, K. W. Theoretical interpretations of learning. *In* Stevens, S. S. (Ed.), *Handbook of experimental psychology*. New York: Wiley, 1951.

Spence, K. W., and Lippitt, R. An experimental test of the sign-gestalt theory of trial and error learning. *J. exp. Psychol.*, 1946, 36, 491-502.

Thorndike, E. L. *Educational psychology*. New York: Teachers College, Columbia Univ., 1913.

Thorndike, E. L. *Human learning*. New York: Century, 1931.

Thorndike, E. L., *et al. The fundamentals of learning*. New York: Teachers College, Columbia Univ., 1932.

Tolman, E. C. *Purposive behavior in animals and men*. New York: Century, 1932.

Tolman, E. C. There is more than one kind of learning. *Psychol. Rev.*, 1949, 56, 144-155.

Tolman, E. C., Hall, C. S., and Bretnall, E. P. A disproof of the law of effect and a substitution of the laws of emphasis, motivation and disruption. *J. exp. Psychol.*, 1932, 15, 601-614.

Tolman, E. C., and Honzik, C. H. "Insight" in rats. *Univ. Calif. Publ. Psychol.*, 1930, 4, 215-232.

Von Förster, H. *Das Gedächtniss: eine quantenphysikalische Untersuchung*. Vienna: Franz Deuticke, 1948.

Watson, J. B. The effect of delayed feeding upon learning. *Psychobiology*, 1917, 1, 51-60.

Wendt, G. R. An interpretation of inhibition of conditioned reflexes as competition between reaction systems. *Psychol. Rev.*, 1936, 43, 258-281.

Wolfe, J. B. Effectiveness of token-rewards for chimpanzees. *Comp. Psychol. Monogr.*, 1936, 12, No. 60.

Woodworth, R. S. Reinforcement of perception. *Amer. J. Psychol.*, 1947, 60, 119-124.

III

CONDITIONED RESPONSE LEARNING

INTRODUCTION

SINCE the results of Pavlov's (1927) work have become generally known, there has been an enormous amount of research activity in the field of conditioned response learning. There are a number of reasons for the popularity of this approach to the study of learning. From the standpoint of this book, the most important of these is that conditioning techniques permit the relatively precise determination of various relationships which we can assume to be fundamentally true of the learning process in general. That this assumption is entirely correct is, of course, debatable, as we have seen in Chapter II. On the other hand, some of the processes and relationships involved in conditioned response learning undoubtedly apply to the learning of more complex habits and skills. In consequence, we find that the results of conditioning research have been a fruitful source for theoretical concepts used in the explanation of more complex forms of learning.

In view of the enormous literature on conditioned response learning, and in view of the fact that much of this literature falls slightly outside the scope of this book, it is impossible thoroughly to review conditioning studies here. At best, we can hope to describe some of the important conditioning situations and to describe some of the important relationships which have been determined in these situations. It will not be feasible, however, to cite very many individual studies of conditioned response learning.

THE CLASSICAL CONDITIONED RESPONSE

Pavlov's (1927) situation for the study of conditioned response learning in dogs is so well known that a detailed description of it is probably unnecessary. Briefly, it involved pairing an originally

neutral stimulus with one which had the capacity to evoke the natural reflex of salivation. Thus, a tone, presented contiguously in time with meat powder introduced into the dog's mouth, eventually acquired the capacity to elicit the salivary response. Before such training, of course, tones do not have this property. This type of learning situation is often described as being a *classical conditioning* situation. If we move from the specific characteristics of Pavlov's conditioning situation to the general and essential features of it, we may note that the classical conditioning situation consists of (a) an originally neutral stimulus called a *conditioned stimulus,* (b) a stimulus which has the characteristic of evoking one of the natural reflex responses of the learner, termed an *unconditioned stimulus,* (c) the reflex response to this unconditioned stimulus known as an *unconditioned response,* (d) the pairing together in time of the conditioned and unconditioned stimuli, and (e) the eventual occurrence of a response which closely resembles the unconditioned response, but made in response to the conditioned stimulus, known as a *conditioned response.*[1] It is apparent that classical conditioned response learning is not limited to salivary conditioning. Rather, any of the natural reflexes of the body may probably be conditioned. It should be noted, however, that some reflex responses are more "conditionable" than others. This is unquestionably due, in part, to the relative adequacy and inadequacy of the laboratory techniques involved in the study of different kinds of reflexes, but there are probably differences also in the "conditionability" of different reflexes which depend upon characteristics of the learner rather than upon the skill of the experimenter. The important thing to note in this connection, however, is that the types of response which may be learned by the techniques of classical conditioning are limited by the reflex repertoire of the learner.

[1] Elaborate techniques for isolating the learner, for controlling the stimuli, and for measuring the responses which the learner makes have been evolved. An extended discussion of these would require much space and would be somewhat out of place in a work of this kind. It may be said, however, that the higher the degree of isolation of the learner with the consequent removal of extraneous stimulation, and the higher the precision of control over the relevant stimuli, the greater will be the efficiency of learning.

An enormous variety of species, including, of course, the human organism, may be conditioned under the procedures outlined above. Similarly, almost any form of stimulation to which the learner is sensitive may be employed as a conditioned stimulus, the primary restriction being that the conditioned stimulus is not so intense as to arouse responses which interfere with the conditioning process.[2]

It should also be noted that, in the classical conditioning situation, the unconditioned stimulus is presented on every learning trial, regardless of any response made by the learner. In this respect, classical conditioning differs from instrumental conditioning (sometimes called operant conditioning), which is discussed in a later section of this chapter.

Phenomena and Relationships Involved in Classical Conditioning

(A) *The course of acquisition of conditioned responses.* Conditioned reflexes are acquired gradually. Although the rate and final amount of conditioning depend upon the conditions of practice, the general course is a gradual one. Conditions which influence the course of conditioning include the relative massing of the conditioning trials, the intensity of the conditioned and unconditioned stimuli, and the degree of temporal contiguity between them. The nature of the learning curve relating practice to performance varies, also, with the types of measure employed. In general, however, the learning curve is usually either of the negatively accelerated variety or of an S-shaped type. The acquisition process may be interrupted if an extraneous stimulus is introduced during training. Thus, if a dog is being conditioned to salivate to the flash of a light (as a conditioned stimulus), an unfamiliar auditory stimulus introduced into the situation may cause the temporary dropping out of the conditioned response. Such interference is known as *external inhibition.* Its occurrence is one of the facts which require isolation

[2] The reader is referred to Hull (1934) and to Hilgard and Marquis (1940) for discussions of technique and a description of the organisms and responses which have been conditioned.

of the learner and a rigid control of the stimuli involved in the learning process.

(B) *The course of retention of conditioned responses.* The retention of conditioned reflexes has, perhaps, not received as much research attention as this topic deserves. There is every indication, however, that conditioned reflexes are retained at an extremely high level over very considerable periods of time. The lack of systematic information on this point is undoubtedly due to the fact that decrements in conditioned responses are so easily produced experimentally. This is accomplished by presenting the conditioned stimulus for a number of trials without pairing it with the unconditioned stimulus. The resulting disappearance of the conditioned response is known as *experimental extinction.* If, during the retention interval, the learner is protected from stimuli which resemble the conditioned stimulus, little or no decrement in the conditioned response will be obtained. It is, thus, safe to assume that experimental extinction is the primary mechanism by means of which conditioned reflexes are lost.

Ordinarily, experimental extinction occurs gradually according to a negatively accelerated function.[3] Extraneous stimulation, however, can disrupt the extinction process. Such an extra stimulus, if it is of moderate intensity, may produce *disinhibition,* which is a temporary reappearance of the partially extinguished response. If the extra stimulus is of greater intensity, however, a response decrement rather than a response increment may result.

Rate of experimental extinction depends upon a number of conditions. One of the most important of these is the degree of massing of the extinction trials. Although conditioning generally proceeds more rapidly under distributed trials, extinction occurs most rapidly under massed trials.[4]

[3] An exception to this is sometimes found when learning has occurred by massed trials. In this case, the extinction curve shows an initial rise. This "reminiscence" effect is probably due to the disinhibition of "inhibition of reinforcement," which has accumulated during the conditioning trials. The disinhibition effect results from the change in the stimulus complex when the unconditioned stimulus is omitted (cf. Hovland, 1936).

[4] Results obtained by Reynolds (1945b) demonstrate that the effects of massing and distribution in extinction depend upon rate of practice in condi-

Following experimental extinction, if a rest period is introduced, the effects of extinction tend to dissipate and the originally learned response may again be given upon presentation of the conditioned stimulus. This process, known as *spontaneous recovery*, is rarely complete, however. Furthermore, amount and rate of spontaneous recovery can be reduced by *sub-zero extinction*, i.e., by continuing

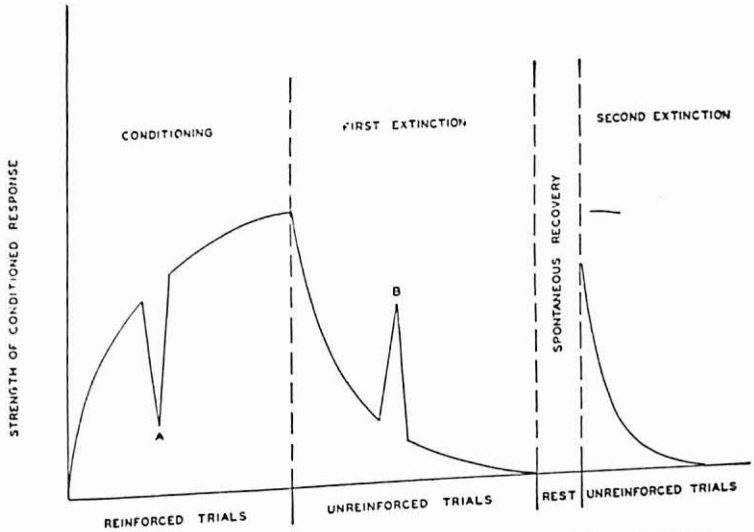

Fig. 6. Some Representative Conditioning Phenomena

Conditioning, extinction, spontaneous recovery, and a second extinction are shown. The curves are schematic, real data being somewhat less regular than these. Note that spontaneous recovery is not complete and that the second extinction is more rapid than the first. The effects of extraneous stimulation during conditioning and extinction (external inhibition and disinhibition) are shown at the points labeled A and B.

to give extinction trials after the measured conditioned response has dropped to zero. Successive series of extinction trials, with recovery occurring between them, result in successively more rapid losses of the conditioned response and successively smaller amounts of spontaneous recovery until, eventually, permanent extinction may be achieved.

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tioning. His findings show that massed extinction trials cause faster extinction when learning has been by distributed practice, but that no difference is obtained between massed and distributed extinction trials when learning has been by massed practice.

Some of these phenomena of conditioning, extinction, and spontaneous recovery are presented schematically in Figure 6. This diagram presents, in idealized form, results which might be obtained in a number of different experiments on conditioning.

(C) *Stimulus generalization.* A conditioned response which has been established so that it may be elicited by a particular conditioned stimulus may also be elicited by other, similar, conditioned stimuli. The magnitude of such a generalized conditioned response depends upon the similarity between the conditioned stimulus used in original training and the stimulus used in the test of generalization. Figure 7 illustrates this gradient of stimulus generalization for the conditioned galvanic skin response in man. Similar gradients may be obtained in other situations.

Concerning the fact of stimulus generalization, little doubt exists. The interpretations of this phenomenon, however, have led to a certain amount of disagreement. This disagreement springs, in part, from the fact that there are two identifiable mechanisms in terms of which stimulus generalization can occur. Following Hull's (1943) terminology, these may be referred to as *primary* and *secondary* stimulus generalization. Primary stimulus generalization depends upon the innate organizing properties of the organism, that is, upon inherent dimensionalization of the afferent results of stimulation. Secondary stimulus generalization, on the other hand, rests upon a previous learning process or a dimensionalization which results from previous experience. In 1946, Lashley and Wade called into question the existence of the primary stimulus generalization gradient. According to this view, all stimulus generalization resulted from the dimensionalization of stimuli in terms of past experience. Without such prior experience, it is held, discrimination would not occur and perfect (i.e., 100 per cent) generalization should result. Hull (1947) has reviewed the evidence on this matter and has argued strongly for the genuineness of the primary stimulus generalization gradient. Later results obtained by Grice (1948b, 1949) and Grice and Saltz (1950) offer support to the position taken by Hull. Furthermore, the fact that no two stimulus situations are ever identical, so that, without primary stimulus generalization, no

learning involving more than a single trial could occur, would seem to weigh heavily in favor of such a concept.[5]

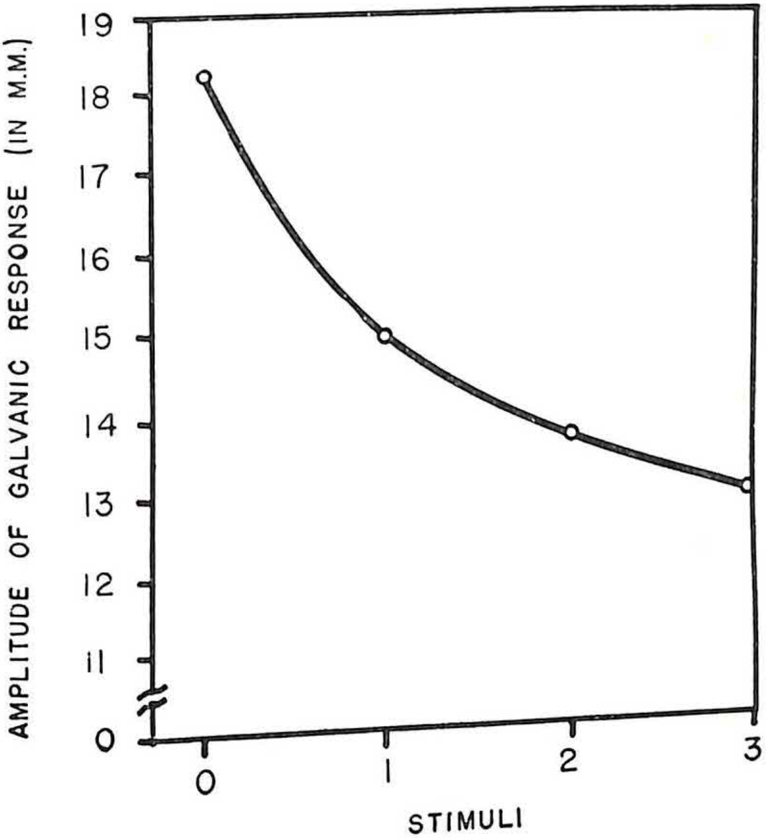

FIG. 7. COMPOSITE CURVE OF SENSORY GENERALIZATION
(From Hovland, *J. gen. psychol.*, 1937, 17, p. 136)

Galvanic skin response to conditioned frequency of tone (o) and to other tones 25, 50, and 75 j.n.d.'s removed in frequency (1, 2, and 3, respectively). Based upon 40 values (2 determinations upon each of 20 subjects) for each point.

Stimulus generalization, as a phenomenon, applies not only to the effects of training, but also to the effects of extinction. The gen-

[5] One could, of course, adopt a stimulus-generalization-by-identical-elements point of view similar to the one taken by Guthrie (1935). This, however, places one in the position of having to define what these elements may be. The student is urged to read Hull's discussion of these matters on pages 188-197 of his (1943) book.

ralization gradients for excitation and extinction are, almost cer-ainly, of the same general form.

(D) *Time intervals in conditioning.* The classical conditioning tuation is an excellent one for the study of time relations between timulus and response. Such studies are, in effect, concerned with ne classical law of contiguity. In terms of the experimental oper-tions which are performed, however, it is the interval between ne conditioned and the unconditioned stimuli rather than the nterval between stimulus and response which is varied. There are number of different types of temporal arrangements which may e employed. These are usually identified as:

(1) *Simultaneous conditioning,* wherein the conditioned stimulus s initiated simultaneously with the unconditioned stimulus. Ac-ually, this is a temporal relationship which is unfavorable for the ccurrence of conditioning. Usually, therefore, the term, simul-aneous conditioning, is used also to denote the relationship wherein he conditioned stimulus precedes and overlaps with the uncon-litioned stimulus by a short time interval, usually of less than five econds (cf. Hilgard and Marquis, 1940).

(2) *Backward conditioning* involves the administration of the conditioned stimulus after the unconditioned stimulus. Although here is some disagreement regarding whether or not *any* condi-ioning results from this procedure, all investigators are in agree-nent that it is an extremely inefficient condition of practice. Small amounts of backward conditioning have been obtained by some nvestigators, but there is a distinct possibility that this may have been due to the occurrence of pseudo-conditioning.[6]

(3) *Delayed conditioning* occurs when the conditioned stimulus s initiated sometime before the unconditioned stimulus and is al-

[6] An experimental test of this possibility would involve the use of two groups. n both groups, neutrality of the conditioned stimulus would be tested. Then Group I would receive a number of backward conditioning trials while Group II vould receive an equal number of presentations of the unconditioned stimulus alone. Subsequent equality of the groups in respect to amount of "conditioned esponse" elicited by the conditioned stimulus would demonstrate that back-vard conditioning could be accounted for by pseudo-conditioning.

lowed to continue until the unconditioned stimulus is administered. Under these circumstances, a number of characteristic phenomena may be noted. Very early in training, there is, of course, no learned response to the conditioned stimulus. As training progresses, however, and the conditioned response is formed, it tends to occur as soon as the conditioned stimulus is initiated. With continued training, however, the conditioned response tends to move "backward" in time, so that it occurs shortly before the time when the unconditioned stimulus is given. This shift in the time of occurrence of delayed conditioned responses involves the building up of *inhibition of delay*. It may be shown that actual suppression of the conditioned response occurs during this delay period, for, if an extraneous stimulus is administered during this time, disinhibition occurs and the response is given immediately.

(4) *Trace conditioning* is very like delayed conditioning, the only difference being that the conditioned stimulus is initiated *and terminated* before the unconditioned stimulus is administered. The same phenomena, involving the inhibition of delay, occur during trace conditioning as occur during delayed conditioning.[7] In both cases, it should be noted, conditioning becomes more difficult as the time interval between the onset of the conditioned and unconditioned stimuli increases.

Some of these temporal relationships and phenomena are illustrated in Figure 8, which shows the relationship between frequency of conditioned finger withdrawal and the temporal arrangement of the conditioned and unconditioned stimuli. This graph is not entirely adequate as an illustration because it is based upon human avoidance training rather than upon classical conditioning. It is also probable that the points representing forward conditioning are somewhat too low. Nevertheless, the general trend is well illustrated. It will be seen, for example, that the relationship of greatest

[7] Here, we are referring to delayed and trace-conditioned responses where fairly long time intervals are involved (more than five seconds, for example). With shorter time intervals, as in "short trace" conditioned responses, there is insufficient time for these phenomena to become manifest.

efficiency is that of short forward conditioning, and that efficiency falls off with great rapidity on either side of this optimal point.[8]

(E) *Differential conditioning.* This is a simple form of discrimination learning. Suppose that a conditioned salivary response has been established using a tone of 1000 dv as a conditioned stimulus. Because of stimulus generalization, we would expect a salivary response to be elicited by a variety of other tones, tones of 900, 800, 700 cycles, and so on, the amount of response decreasing as the difference between the test stimulus and the original training stimulus

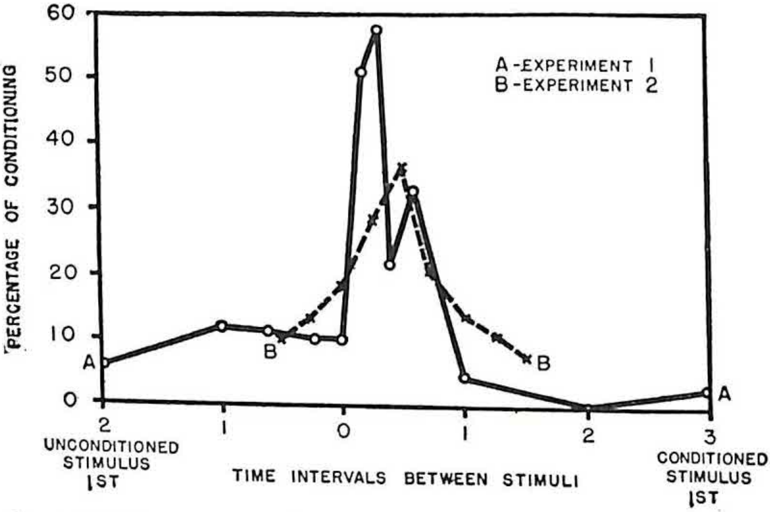

FIG. 8. CONDITIONING AS A FUNCTION OF TIME INTERVAL BETWEEN STIMULI (From Wolfle, *J. gen. Psychol.*, 1932, 7, p. 90)

increased. The fact of this stimulus generalization gradient is, in a sense, evidence of discrimination. However, we may wish to set up a very precise discrimination reaction in the learner. We may wish, for example, to be able to elicit a strong conditioned response using a conditioned stimulus of 1000 dv and no response at all using a conditioned stimulus of 800 dv. This may be accomplished by presenting the two conditioned stimuli an equal number of times in random order under circumstances where the 1000-cycle tone is

[8] For additional information concerning this relationship the student should consult Bernstein (1934), Kappauf and Schlosberg (1937), and Reynolds (1945a).

always followed by reinforcement and the 800-cycle tone is never followed by reinforcement. (We may, if we wish, establish a conditioned response to the 1000-cycle tone before we start differential training. It is also more efficient, in some cases, to start differential training with an easy discrimination, say between 1000 dv and 200 dv. and gradually make the discrimination more difficult.) By following this procedure, we are extinguishing the generalized response to the 800-dv tone while keeping the response to the 1000-dv tone at a high level of strength. (Reinforcement of the positive stimulus is desirable, otherwise the extinction of the 800-dv tone will generalize to the 1000-dv tone and cause a decrement in the response to that stimulus.) By following such procedures, quite fine discriminations can be established. It is worth noting, in passing, however, that this procedure can be carried only so far. If we require the animal to make discriminations which are too fine, the discrimination training breaks down and the animal exhibits disorganized behavior which has become known as "experimental neurosis." [9]

(F) *Conditioned inhibition.* A stimulus which has lost its power to elicit a conditioned response because it has been extinguished or because of differential conditioning seems to acquire inhibitory properties. Such stimuli are known as *conditioned inhibitors*, and have the power to inhibit other conditioned responses. Thus, in the example noted in (E), above, the 800-dv tone becomes a conditioned inhibitor. If this stimulus is now presented simultaneously with a conditioned stimulus which ordinarily elicits a conditioned response, there will be a decrement in this conditioned response.

(G) *Higher-order conditioning.* One other feature of classical conditioning merits our attention here. This is the phenomenon of higher-order conditioning (cf. Frolov, as reported in Pavlov, 1927).

[9] That experimental neurosis is not entirely due to forcing fine discriminations may be seen from the work on partial reinforcement (cf. Chap. VII). Here the learner is required, in effect, to make a discrimination which is infinitely fine, yet no breakdown of behavior occurs. On the contrary, learning may be quite efficient under conditions of partial reinforcement. This discrepancy may result from the fact that the "experimental neurosis experiment" typically starts with a possible discrimination and works toward an impossible one whereas, in partial reinforcement experiments, the discrimination is "impossible" from the outset.

This type of conditioning involves using a learned conditioned stimulus—conditioned response relationship as an unconditioned stimulus—unconditioned response basis for a second conditioning experiment. Thus, a dog may be conditioned to salivate to a flash of light, meat serving as the unconditioned stimulus. Now, if the flash of light (which, through conditioning, elicits salivation) is paired with a new conditioned stimulus, say, a tone, conditioning may occur even though tone has never been paired with a proper unconditioned stimulus (meat). The occurrence of higher-order conditioning is of obvious importance as a device for explaining the enormous complexity of psychological development. (This explanation suffers, somewhat, from the fact that orders beyond the second are established, if at all, only with the greatest difficulty, while orders beyond the third may not be established at all, at least with dogs). It is, however, also important as being a simple example of the operation of *secondary reinforcement.* Since secondary reinforcement is discussed, at length, in another portion of this book, we shall do no more than indicate the relevance of this concept here.[10]

A word may be said concerning Pavlov's theoretical interpretation of conditioning phenomena. Pavlov (1927) formulated a simple theoretical structure which accounted for the facts of classical conditioning which have been presented here in terms of three concepts, *excitation, internal inhibition,* and *external inhibition.* The first of these, excitation, was conceived to develop gradually as a function of the number of reinforced trials (trials where both the conditioned and the unconditioned stimuli were presented) and to be relatively permanent. Both inhibitory influences acted to negate excitation. External inhibition, or inhibition caused by extraneous stimulation, could cause a temporary suppression of the conditioned response. This occurred, presumably, because the unaccustomed stimulus

[10] Many other phenomena of classical conditioning have been omitted from this discussion. Temporal conditioning and summation are examples of such phenomena. The reader is again reminded that a thorough coverage of conditioning is impossible in the space we are able to allot to it and is referred to Pavlov (1927), Hilgard and Marquis (1940), and to the voluminous experimental literature on this subject.

elicited investigatory behavior. Internal inhibition, presumably, developed during both reinforced and unreinforced trials, tended to dissipate in time, and was more subject to the disrupting influence of external inhibition than was excitation. Internal inhibition was also conceived to develop in the delay period of long delay and trace-conditioned responses where it was known by a special name, the inhibition of delay. By means of these concepts, and a few additional assumptions, a very considerable number of classical conditioning phenomena may be explained.[11] It should be noted, however, that the acceptance of these concepts does not necessarily imply an acceptance of the theory of the action of the nervous system which Pavlov also formulated.

Subsequent modifications and elaborations of Pavlovian theory have been devised, mainly by Clark Hull and his collaborators. Hull's (1943, 1950) postulate systems allow for a much more precise, quantitative prediction of conditioned response phenomena than is possible with Pavlov's original theory.

PSEUDO-CONDITIONING

Grether (1938) noted an example of what he termed "pseudo-conditioning." This occurs whenever an originally neutral stimulus elicits a conditioned response following a number of *unpaired* presentations of the unconditioned stimulus. For example, Grant and Dittmer (1940) demonstrated pseudo-conditioning of the galvanic skin response. Shock was used as an unconditioned stimulus while a vibratory stimulus applied to the skin served as a conditioned stimulus. After a number of presentations of the shock, *alone*, the vibratory stimulus yielded a galvanic skin response (which it did not have the power to do before this training). This effect has been demonstrated in a considerable number of experiments and may enter as a source of contamination in a great many more. For example, we might expect that final performance in many condi-

[11] Examples of such phenomena are (1) conditioning, (2) experimental extinction, (3) external inhibition as a phenomenon, (4) differential conditioning, (5) the differential effects of massed and distributed conditioning and extinction trials, (6) the time of occurrence of conditioned responses in delayed and trace conditioning, and (7) the phenomenon of disinhibition.

tioning studies is a function of true conditioning resulting from a pairing of the conditioned and unconditioned stimuli, and also of pseudo-conditioning resulting from presentation of the unconditioned stimulus. This phenomenon is important because it appears to contradict the basic theoretical assumption which is usually made, that the conditioned and unconditioned stimuli must be presented in some degree of temporal contiguity if learning is to occur. May (1949) has outlined a relatively complete theory of pseudo-conditioning which reconciles the experimental findings with the theoretical assumptions regarding contiguity. According to this view, pseudo-conditioning is a function of six factors or conditions. Perhaps the most important of these is the assumed relationship between strength of pseudo-conditioning and the similarity appertaining between the conditioned and the unconditioned stimuli. According to this assumption, the unconditioned stimulus acquires habit strength, during training, to elicit its own unconditioned reaction. Similar stimuli are then endowed with the capacity to elicit this response by means of stimulus generalization. For a detailed statement of this, and the other five assumptions, plus a detailed review of the relevant experimental facts, the reader should consult May's paper.

INSTRUMENTAL CONDITIONING

In the classical conditioning situation, it will be recalled, Pavlov's dogs received the reinforcing stimulus on every training trial, regardless of any response they made. In other words, the meat powder was administered whether or not the dog showed evidence of making the learned or to-be-learned response. Under the procedures of instrumental conditioning, however, the to-be-learned response must occur before the reinforcement is administered. In other words, the learned response is *instrumental* in securing the reinforcement. Such learned responses may be termed *instrumental acts.* Because, in other respects, the instrumental conditioning situation may resemble the classical conditioning situation very closely, the distinction between these two forms of learning has not always been made with accuracy. Thus, there would be considerable justi-

fication for classifying as instrumental acts many of the conditioned responses which are commonly regarded as being classical in nature. Conditioned eyelid closure to an air-puff, for example, may be classified as an instrumental act because the anticipatory closure of the lid prevents the puff of air from striking the cornea of the eye. Even the classical salivary conditioned response might be considered instrumental if one were to assume that the anticipatory salivation to the conditioned stimulus enabled the dog to gain greater or more immediate reinforcement from the meat powder which served as an unconditioned stimulus. On the other hand, many of the instrumental learning situations show little resemblance to classical conditioning situations. For example, the conditioned stimulus which is such a prominent feature of the classical conditioning situation may, under instrumental conditioning procedures, be omitted altogether as a formal feature of the learning situation. Instead of learning relatively simple reflex responses, the learner may be required to learn quite complex and involved patterns of behavior in instrumental conditioning. If, at one end of the continuum, instrumental conditioning blends into classical conditioning, so, at the other end, instrumental conditioning merges into problem-solving and trial and error learning situations. On the whole, it is probably best to confine the term, instrumental conditioning, to learning situations which possess the following two characteristics: (1) the act to be learned must be instrumental in securing the reinforcement, and (2) the act to be learned must be relatively high upon the learner's hierarchy of responses, that is, discovery of the correct response should occupy a relatively minor portion of the total practice period.

The Nature of Reinforcement in Instrumental Conditioning

In the classical conditioning situation, a reinforcing stimulus is one which elicits a reflex response of the organism, either because it is the biologically adequate stimulus for that reflex, or because of prior conditioning, as in the case of higher-order conditioning. In the case of instrumental conditioning, however, the nature of

the reinforcing stimulus or state of affairs is much more complex, and, indeed, has been the subject of a considerable amount of theoretical dispute.[12] Without taking sides in this dispute, it is probably not possible to make a clear-cut definition concerning reinforcement. However, a relatively neutral definition of reinforcement, from the standpoint of theory, has been offered by Thorndike (1931), who defines it in the following terms:

"By a satisfying state of affairs is meant one which the animal does nothing to avoid, often doing things which maintain or renew it."

It will be noted that such a definition includes many recognizably different states of affairs. These might include:

(1) Reduction of the intensity of biological, appetitive drives, such as hunger, thirst, sex, and the like.
(2) The attainment of stimuli which have previously been associated with primary drive reduction. Such stimuli are called secondary reinforcing stimuli.
(3) The reduction in intensity of a painful or noxious stimulus, such as an electric shock.
(4) The reduction in intensity of a learned motive, such as fear.
(5) The attainment of certain stimuli or substances which, although they reduce no known drive, and although they have not been associated in the past with drive reduction, nonetheless appear to be sought by various organisms.[13]

This great variety of possible reinforcing states, as well as the freedom which the instrumental conditioning situation gains because the learned response need not duplicate one of the native reflexes, has given rise to a large number of experimental situations of this type. In general, these specific situations may be classified under one of three headings, depending upon the conditions of reinforcement. These are:

(A) *Reward training.* In reward training, the instrumental act which is learned secures for the learner some form of positive reinforcement. By positive reinforcement is meant one which would be classified under headings 1, 2, or 5 in the preceding paragraph.

[12] The reader is referred to Chapters II and VII.
[13] An extended discussion of these mechanisms will be found in Chapter VII.

A considerable number of reward training situations have been developed. The best known of these, perhaps, is the Skinner box.[14] This is a simple problem box used for the study of learning in small animals. Commonly, the apparatus consists of a small, barren chamber in which the animal is confined, a small lever or bar which may be depressed by the animal, and an arrangement for giving reinforcement in the form of food pellets, crushed food, or water. The animal receives reinforcement whenever it depresses the bar (during training). Since the bar is a prominent part of the apparatus, the bar-pressing response usually is high on the animal's response hierarchy. Nevertheless, during the initial stages of training, it is often desirable to increase the probability that the bar will be responded to by smearing it with food or by other means. The apparatus may be arranged so that reinforcement follows immediately upon depression of the bar, or after any desired interval of time. Amount of reinforcement may be varied by loading the food magazine with food pellets of different sizes. If desired, other stimuli may be administered to the animal, either at the time of bar depression or at a time chosen by the experimenter. Typically, the Skinner box is equipped with an automatic recording device which makes a record of the time of each bar-pressing response and of the number of such responses. Similar problem boxes have been developed for use with other animals. Thus, Guthrie and Horton (1946) have used a problem box for cats in which the instrumental act consists of displacing a vertical lever which protrudes from the floor of the box. When this lever is moved, the door of the box is automatically unlatched so that the animal may receive reinforcement in the form of a bit of food placed outside the door. Other types of problem box may be more complex with respect to the nature of the behavior to be learned. Thus, problem boxes of the type used by Thorndike (1898) in his pioneer study of the learning behavior of kittens, approach a trial-and-error type of learning situation in that the discovery of the correct response plays a rather prominent role in the learning process. One other type of reward training situation should be mentioned. This is the "chimp-o-mat,"

[14] Named, of course, for its inventor, Prof. B. F. Skinner.

an automatic vending device which was developed for the purpose of studying secondary reinforcement in chimpanzees. This apparatus resembles a coin-operated vending machine in its functioning, dispensing, as it does, small amounts of food when a poker chip or similar token is inserted in the slot. If this machine is placed in a chimpanzee's cage, the ape quickly learns to insert the tokens into the machine in order to secure the food reward. It is interesting to note that, under these circumstances, the poker chips become sought-after objects (i.e., reinforcing stimuli) for the chimpanzee. We will not discuss this learning of secondary reinforcement at the present time, however, since more extended treatment will be given later in the book (Chapter VII).

In the instrumental conditioning of human subjects, various reward training devices have been employed. Because of the fact that instrumental conditioned responses are, by definition, fairly simple, obvious responses, a fundamental objection may be voiced against the use of such learning situations in the study of human learning. The difficulty lies in the fact that responses which may be learned in instrumental conditioning situations are typically quite subject to mediation and control by language habits. This difficulty may be reduced or avoided in a number of ways. The experimenter may make the problem more difficult to solve, thus, in some cases, reducing the factor of verbal control. This solution is not particularly satisfactory because, by increasing the difficulty of the problem, the experimenter typically moves from the field of instrumental conditioning into the area of trial-and-error learning. Another solution is to attempt to find a simple response which may be conditioned and which does not appear to be readily subject to verbal control. Greenspoon (reported by Dollard and Miller, 1950) has reported some success in the conditioning of language patterns by this method. A third solution is to study instrumental reward training with subjects who do not have the ability to use language, that is, with very young children or with mentally deficient adults. A recent study by Fuller (1949) illustrates how the instrumental reward training technique may be used to condition a simple response in an adult vegetative idiot.

(B) *Escape training.* Here the instrumental act permits the learner to terminate a painful or noxious stimulus such as an electric shock. It does not, however, enable him to avoid this stimulus altogether. This learning situation has been infrequently used. Since, in escape training, the learner may not avoid the noxious stimulus altogether, but is able to escape from it once it has occurred, learning consists in an increase in the efficiency with which the learner terminates the noxious stimulus. For example, suppose an animal to be placed in a box which has an electric grid on the floor. Half of the grid is electrified, so that if the animal moves into the "incorrect" half of the box he receives an electric shock. Further suppose that the experimenter arranges matters so that the two halves of the box may be alternately charged, the changes occurring at unpredictable intervals. Under these circumstances, the animal cannot learn to avoid the shock altogether. The most efficient form of behavior he may acquire is to move as rapidly as possible to the "safe" side of the box the moment the shock is shifted. Very few experiments have been exclusively concerned with escape training, although avoidance training, as we shall see, necessarily involves a period of escape training. Recently Miller (1948) has used escape from a secondary drive (fear) as a reinforcing state of affairs.

(C) *Avoidance training.* If the instrumental act serves to prevent the occurrence of a noxious stimulus, we may speak of avoidance training. A typical avoidance situation is illustrated in the experimental technique employed by Finch and Culler (1934). A dog was placed in a conditioning harness with one of his feet resting on an electric grid. A conditioned stimulus, in this case a tone, was given, followed a few moments later by the electrification of the grid. During the early stages of practice, the animal escaped the shock by lifting his foot from the grid as soon as the shock was administered. After continued practice, however, the dog learned to lift his foot from the grid in response to the conditioned stimulus, thus avoiding shock altogether. An interesting feature of avoidance conditioning is its relative insusceptibility to experimental extinction. Numerous studies confirm this effect, which differs greatly from the extinction phenomenon as found in classical conditioning or in

instrumental reward training. In order to appreciate the extraordinary resistance of avoidance training to extinction, it must be realized that, as soon as the animal passes beyond the stage of escape learning and begins to lift his foot before shock is initiated, each trial is, in a sense, an extinction rather than a training trial. During training, of course, if the animal fails to lift his foot at the occurrence of the conditioned stimulus, the electric shock serves quickly to reinstate the withdrawal response. But, even if the experimenter turns off the current to the electric grid entirely, the animal continues to lift its leg, oftentimes for hundreds of trials (cf. Finch and Culler, 1934; Brogden, Lipman, and Culler, 1938).

Mowrer (1939) has advanced an hypothesis to account for this failure of extinction to occur. According to this view, during avoidance training the animal learns two separate responses. The first of these is the measured response of lifting the leg when the conditioned stimulus is given. The second response is a fear reaction, which is also elicited by the conditioned stimulus. After learning has progressed to some extent, the animal makes both of these responses upon each occurrence of the conditioned stimulus. It is to be noted that both responses are initiated by the conditioned stimulus and both terminate, *i.e.*, the leg is replaced and the fear subsides, when the conditioned stimulus is turned off. Fear reduction, thus, occurs immediately after the leg-lifting response, and, according to Mowrer's hypothesis, this fear reduction serves as the reinforcement which maintains the strength of the leg-withdrawing response. Since, on each trial, fear is reduced immediately following leg withdrawal, this response is reinforced by fear reduction in the absence of the original unconditioned stimulus, shock. Independent verification of portions of this hypothesis may be found in the experiments of Miller (1948) and Solomon and Wynne (1950). Miller demonstrated that reduction of an acquired fear motive could serve as a reinforcement in the sense that the animal would learn an instrumental act in order to escape from a fear-arousing stimulus. Solomon and Wynne's experimental results have an even more direct bearing upon the Mowrer hypothesis. They found that conditioned avoid-

ance responses were easily extinguished in sympathectomized dogs, that is, in dogs in whom the capacity for fear behavior was, presumably, greatly reduced.

But the Mowrer hypothesis, illuminating as it is, merely pushes back the problem of extinction for one step. The problem of why the foot-lifting response doesn't extinguish may be solved, but another problem is raised. That problem is this: why, if the avoidance response is maintained by the reduction of a learned fear response, does not the fear response, itself, extinguish? There may be two plausible explanations for this. One is the assumption that fear responses do not extinguish simply through non-reinforcement, but may only be removed through a counter-conditioning procedure. This explanation accounts for the facts well enough, it is true, but it is merely a convenient assumption designed to account for those facts. A second explanation follows from a strict interpretation of reinforcement theory. This explanation would hold that the fear response is not extinguished because of the reinforcement which is derived from its own reduction. If the fear response is a learned response, and if fear reduction is a reinforcing state of affairs, then the tendency for the conditioned stimulus to arouse fear might logically receive reinforcement from the fear reduction just as does the connection between the conditioned stimulus and the leg-withdrawal response.[15]

Problems Most Conveniently Attacked by Instrumental Learning Techniques

In most respects, the general phenomena of classical conditioning may also be obtained in the various instrumental conditioning situations. Thus, learning, extinction, spontaneous recovery, stimulus

[15] The similarity between avoidance conditioning and repressive forces involved in personality disorders has been noted by a number of writers. The possibility that a secondary drive, such as fear, may be able to strengthen or maintain itself by its own reduction provides a mechanism by means of which the relative permanence of these drives may be assured. If this explanation is correct, it offers an attractive device by means of which the relative stability of psychological organization may be explained.

generalization, and so on, may be observed under proper circumstances. It should be noted, however, that each technique is particularly advantageous for the study of certain types of problems. Instrumental conditioning is peculiarly well adapted to the study of particular aspects of the learning process which might not be so advantageously studied by other methods. Especially important among these are the following:

(A) *The effect of motive-incentive conditions upon learning and performance.* There are many specific problems contained within this general statement. For example, given a certain fixed amount of previous learning, does an increase or decrease in the motivational level effect the performance of the learned act? Another problem might be, if practice is given under different amounts of motivation, may different levels of performance be observed under conditions of a subsequent test wherein motivational level is equated for all learners? A third problem concerns the effect of the magnitude of the reinforcing agent upon the learning process. A fourth problem might be to determine the amount of interaction, if any, between the amount of motivation present during learning and the magnitude of the reinforcing agent. Because amount and immediacy of reinforcement are easily controlled, the instrumental conditioning situation offers an excellent opportunity to attack experimental problems such as those which are outlined above. However, the reader should not feel that this is an exhaustive list of problems in this area. Rather, an almost infinite number of problems relating to specific motivational states and specific incentive objects await experimental investigation.

(B) *Delay of reinforcement.* What is the effect upon learning of introducing a period of delay between the making of the correct response and the giving of the reinforcement? This problem may not be studied in the classical conditioning situation because the reinforcing stimulus is also the one which produces the to-be-learned response. Although the instrumental conditioning situation is well adapted to the study of this problem, important research in this area has been accomplished in other types of experimental

situations. In particular, the maze-learning situation has been much employed for this purpose.[16]

(C) *The learning and consequences of secondary motivation.* Under what conditions may motives be learned? Having learned them, what role do they play in subsequent performance and how may they influence subsequent learning? Because of the variety of possible reinforcing states of affairs which may be used and because of the general adaptability of the instrumental conditioning technique, it is well adapted for the study of problems in this area.[17]

(D) *Secondary reinforcement.* Does a stimulus which is frequently associated with primary motive reduction acquire the property to serve as a reinforcing agent for the learning of other acts? Under what circumstances may a stimulus acquire such properties? We have already noted that secondary reinforcement may be studied in the classical conditioning situation where the work on higher-order conditioning is relevant (Frolov, as reported in Pavlov, 1927). Other studies of secondary reinforcement have employed maze or discrimination learning techniques (Grindley, 1929; Denny, 1946; Grice, 1948; Ehrenfreund, 1949). The bulk of the work on secondary reinforcement has been accomplished in the instrumental conditioning situation, however.

Other types of problems, of course, have been attacked by instrumental conditioning techniques. The listing, above, is not intended to be inclusive, but merely to indicate the adaptability of this technique to a variety of problems and to enumerate some of the more important problems which may be most advantageously studied by this method.

[16] The reader is reminded that important studies on this subject have used other techniques, and is referred to the papers on the goal gradient in maze learning by Yoshioka (1929), Hull (1932, 1934), and Grice (1942). Grice (1948a) has also studied delay of reinforcement using a discrimination learning technique.

[17] Again, an exception must be noted. Brown (1942a, b) has used a single alley maze situation for the study of conflict behavior involving secondary motivation.

The Importance of Conditioned Response Learning

Conditioned response learning is important for an understanding of human behavior in a number of ways. In the first place, many of the principles which are used to explain more complex forms of learning, so characteristic of the human adult, are derived from conditioning experiments. For example, Melton and Irwin (1940) attempt to explain the complex phenomenon of retroactive inhibition partially in terms of the concept of experimental extinction, a concept derived from studies of classical conditioning. As another example, Hull's (1930, 1939) analysis of trial-and-error learning may be cited. In this case, we have an attempt to explain a complex form of learning in terms of such simple conditioning concepts as excitation, extinction, and spontaneous recovery. Secondly, while we may understand the importance of certain conditioning concepts in the explanation of very complex forms of behavior, in many cases the nature of the explanatory concept is best worked out in simple conditioning situations. Thus, we may be assured that secondary motives play an enormously important role in the determination of human learning and activity at the very most complex levels, yet, in these complicated situations we may not be able to isolate the variables of which secondary motive formation is a function. Similarly, extinction may be used partially to explain variability of behavior in trial-and-error learning situations, but it would be extremely difficult to obtain a clear notion of the nature of experimental extinction from studying trial-and-error learning, alone.

It is also true that a considerable proportion of the everyday acts of human beings are learned under the general conditions of classical and instrumental conditioning. We may feel fairly certain that many emotional habits, fears for example, are acquired by the association of a previously neutral stimulus with an unconditioned stimulus of a painful nature. In like manner, many of the early acquired responses of the human infant are instrumental acts learned in consequence of food reinforcement or the reinforcement derived from reduction in discomfort. It is to be expected that this type of learning is more frequently found in the infant than in the

adult, owing to the mediation of behavior by language, which plays such a tremendously important role in the latter case. Nevertheless, both classical and instrumental conditioned responses continue to be formed in the adult, perhaps in unsuspectedly large numbers.

REFERENCES

Bernstein, A. L. Temporal factors in the formation of conditioned eyelid reactions in human subjects. *J. gen. Psychol.*, 1934, 10, 173-197.

Brogden, W. J., Lipman, E. A., and Culler, E. The role of incentive in conditioning and extinction. *Amer. J. Psychol.*, 1938, 51, 109-117.

Brown, J. S. The generalization of approach responses as a function of stimulus intensity and strength of motivation. *J. comp. Psychol.*, 1942a, 33, 209-226.

Brown, J. S. Factors determining conflict reactions in difficult discriminations. *J. exp. Psychol.*, 1942b, 31, 272-292.

Denny, M. R. The role of secondary reinforcement in a partial reinforcement learning situation. *J. exp. Psychol.*, 1946, 36, 373-389.

Dollard, J., and Miller, N. E. *Personality and psychotherapy*. New York: McGraw-Hill, 1950.

Ehrenfreund, D. Effect of a secondary reinforcing agent in black-white discrimination. *J. comp. physiol. Psychol.*, 1949, 42, 1-5.

Finch, G., and Culler, E. Higher order conditioning with constant motivation. *Amer. J. Psychol.*, 1935, 47, 656-662.

Fuller, P. R. Operant conditioning of a vegetative human organism. *Amer. J. Psychol.*, 1949, 62, 587-590.

Grant, D. A., and Dittmer, D. G. A tactile generalization gradient for a pseudo-conditioned response. *J. exp. Psychol.*, 1940, 26, 404-412.

Grether, W. F. Pseudo-conditioning without paired stimulation encountered in attempted backward conditioning. *J. comp. Psychol.*, 1938, 25, 91-96.

Grice, G. R. An experimental study of the gradient of reinforcement in maze learning. *J. exp. Psychol.*, 1942, 30, 475-489.

Grice, G. R. The relation of secondary reinforcement to delayed reward in visual discrimination learning. *J. exp. Psychol.*, 1948a, 38, 1-16.

Grice, G. R. The acquisition of a visual discrimination habit following response to a single stimulus. *J. exp. Psychol.*, 1948b, 38, 633-642.

Grice, G. R. Visual discrimination learning with simultaneous and successive presentation of stimuli. *J. comp. physiol. Psychol.*, 1949, 42, 365-373.

Grice, G. R., and Saltz, E. The generalization of an instrumental response to stimuli varying in the size dimension. *J. exp. Psychol.*, 1950, 40, 702-708.

Grindley, G. C. Experiments on the influence of the amount of reward on learning in young chickens. *Brit. J. Psychol.*, 1929, 20, 173-180.

Guthrie, E. R. *The psychology of learning*. New York: Harpers, 1935.

Guthrie, E. R., and Horton, G. P. *Cats in a puzzle box*. New York: Rinehart, 1946.

Hilgard, E. R., and Marquis, D. G. *Conditioning and learning*. New York: Appleton-Century, 1940.

Hovland, C. I. "Inhibition of reinforcement" and phenomena of experimental extinction. *Proc. nat. Acad. Sci.*, Wash., 1936, 22, 430-433.

Hovland, C. I. The generalization of conditioned responses: I. The sensory

generalization of conditioned responses with varying frequencies of tone. *J. gen. Psychol.*, 1937, 17, 125-148.

Hull, C. L. Simple trial and error learning: a study in psychological theory. *Psychol. Rev.*, 1930, 37, 241-256.

Hull, C. L. The goal gradient hypothesis and maze learning. *Psychol. Rev.*, 1932, 39, 25-43.

Hull, C. L. The factor of the conditioned reflex. In Muchison, C. (Ed.) *A handbook of general experimental psychology*, 382-455. Worcester: Clark Univ. Press, 1934a.

Hull, C. L. The rat's speed-of-locomotion gradient in the approach to food. *J. comp. Psychol.*, 1934b, 17, 393-422.

Hull, C. L. Simple trial and error learning: an empirical investigation. *J. comp. Psychol.*, 1939, 27, 233-258.

Hull, C. L. *Principles of behavior.* New York: Appleton-Century, 1943.

Hull, C. L. The problem of primary stimulus generalization. *Psychol. Rev.*, 1947, 54, 120-134.

Hull, C. L. Behavior postulates and corollaries—1949. *Psychol. Rev.*, 1950, 57, 173-180.

Kappauf, W. E., and Schlosberg, H. Conditioned responses in the white rat. III. Conditioning as a function of the length of the period of delay. *J. genet. Psychol.*, 1937, 50, 27-45.

Lashley, K. S. and Wade, M. The Pavlovian theory of generalization. *Psychol. Rev.*, 1946, 53, 72-87.

May, M. A. An interpretation of pseudo-conditioning. *Psychol. Rev.*, 1949, 56, 177-183.

Melton, A. W., and Irwin, J. McQ. The influence of degree of interpolated learning on retroactive inhibition and the overt transfer of specific responses. *Amer. J. Psychol.*, 1940, 53, 173-203.

Miller, N. E. Experimental studies of conflict. *In* Hunt, J. McV. (Ed.), *Personality and the behavior disorders*, Vol. 1, 431-465. New York: Ronald Press, 1944.

Miller, N. E. Studies of fear as an acquirable drive: Fear as motivation and fear-reduction as reinforcement in the learning of new responses. *J. exp. Psychol.*, 1948, 38, 89-101.

Mowrer, O. H. A stimulus-response analysis of anxiety and its role as a reinforcing agent. *Psychol. Rev.*, 1939, 46, 553-565.

Pavlov, I. P. *Conditioned reflexes.* (Trans. by G. V. Anrep), London: Oxford Univ. Press, 1927.

Reynolds, B. The acquisition of a trace conditioned response as a function of the magnitude of the stimulus trace. *J. exp. Psychol.*, 1945a, 35, 15-30.

Reynolds, B. Extinction of trace conditioned responses as a function of the spacing of trials during the acquisition and extinction series. *J. exp. Psychol.*, 1945b, 35, 81-95.

Solomon, R. L., and Wynne, L. C. Avoidance conditioning in normal dogs and dogs deprived of normal autonomic functioning. *Amer. Psychologist*, 1950, 5, 264. (Abstract.)

Thorndike, E. L. Animal intelligence: an experimental study of the associative processes in animals. *Psychol. Monogr.*, 1898, 2, No. 8.

Thorndike, E. L. *Human learning.* New York: Appleton-Century-Crofts, 1931.

Wolfle, H. M. Conditioning as a function of the interval between the conditioned and the original stimulus. *J. gen. Psychol.*, 1932, 7, 80-102.

Yoshioka, J. G. Weber's law in the discrimination of maze distance by the white rat. *Univ. Calif. Publ. Psychol.*, 1929, 4, 255-284.

IV

INTRASERIAL PHENOMENA

INTRODUCTION

COMPLEX habits which are composed of a number of separate parts are often learned serially, that is, in a particular order of parts. This order becomes an integral part of the habit as a whole. Not only do serially learned habits represent a considerable proportion of experimental situations for the study of learning, but also, in the everyday learning of people, an enormous number of complex habits are acquired in this way. This may be most evident in the case of verbal habits, the recitation of poetry and prose passages, for example, but many non-verbal habits also depend upon a particular sequence of part-responses for their effectiveness. Learning to negotiate a maze path or other spatial pattern, memorizing a musical selection, or acquiring skill in serving a tennis ball are examples of serial habits. Such habits may be defined as complex habits wherein the ordering of the part-responses plays an important role in determining the outcome of the behavior as a whole.

For many purposes, serial habits may be regarded as wholes, and time, trial and error scores may be taken which represent the total acquisition of the habit. There are, however, certain phenomena occurring within this over-all learning. These phenomena are of considerable importance. Not only are they legitimate objects for special investigation, but the understanding of them will aid in the interpretation of the learning of the task as a whole.

A serial habit is usually practiced under the instruction or set to connect each portion of the series to the one which comes immediately after it. Learning is said to have occurred when each item will arouse the next following item. Consider, as an example, the learning of a list of words by the serial anticipation method. If the words in the list are represented by the letters of the alphabet, then A leads

to B, B leads to C, C to D, and so on to the end of the list. Each item in the list (except for the first) serves a dual purpose. It is a response to the preceding item, and it is a stimulus to the item which follows. Early in practice, this stimulation is maintained by external stimuli, i.e., the words presented on the memory drum. When the appropriate responses have been acquired, however, the dependence on this external source of stimulation disappears, the responses themselves become stimuli, and the series may be run off in relative independence of environmental cues. A provocative analysis of many of the phenomena of serial learning, with special reference to maze learning, has been made in a series of theoretical papers by Hull (1930, 1931, 1932, 1934a, 1934b, 1937), which no student of the learning process can afford to ignore. Concerning the rote-serial learning of verbal materials, Hull has made two theoretical-empirical analyses (1935b, 1940). Discussion of portions of these papers is reserved for later sections of this chapter.

The phenomena of serial learning to be discussed in this chapter are: (1) remote associations in the forward direction, (2) associations in the backward direction, and (3) serial position effects.

REMOTE ASSOCIATIONS IN THE FORWARD AND BACKWARD DIRECTIONS

So far, we have noted only the existence of the adjacent, forward associations, the formation of which constitutes the objective of this type of learning. Such associations which operate in a forward direction between the successive items in a series are termed *immediately successive associations*, *adjacent associations*, or *forward associations of zero degree of remoteness*. Adjacent associations, however, may not be the only ones which are formed during practice. The first item in the list may lead not only to the second, but also, on occasion, to the third, fourth, or fifth item. Similarly, the sixth item may occasionally elicit the third or the tenth item as a response. Associations between non-adjacent items are called *remote associations*. If a remote association is formed between one item and another which is farther forward in the series, it is termed a *remote forward association*. Similarly, if one item in a series elicits,

as a response, an item presented earlier in the series, the association is termed a *remote backward association*. It is to be noted that all backward associations, even to the adjacent item, are typically treated as remote associations. Therefore, all remote associations which occur during the learning of a series represent errors in learning. Remote forward associations result in *anticipatory errors*, while backward associations result in *perseverative errors*.

The diagram in Figure 9 is a schematic illustration of adjacent and remote associations. In this diagram, the letters represent the successive items in a series, while the arrows indicate associations between these items. The solid arrows stand for the usual adjacent associations, while the dotted arrows represent remote associations. The dotted arrows above the letters stand for remote forward associations and the dotted arrows below the letters indicate backward

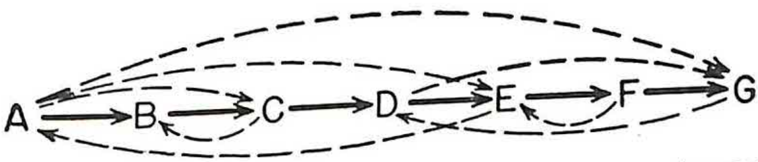

FIG. 9. A SCHEMATIC ILLUSTRATION OF ADJACENT AND REMOTE ASSOCIATIONS

associations. It is obvious that not all of the possible remote associations are illustrated in the diagram. Probably, during the learning of a series, each item becomes connected, to a degree, to each other item. These associations differ markedly in strength, the adjacent forward associations being the strongest. In our discussion, we shall consider remote forward associations first. There are several traditional methods for studying remote forward associations.

The Method of Derived Lists

Ebbinghaus (1885), the first psychologist to make a direct experimental attack upon the problem, invented one of the chief methods for measuring remote associations. By his method of derived lists, the subject first learns a list of items to some criterion. Later he learns a second list, derived from the first. This derivation is made

in such a way that remote associations in the first list, if formed, will facilitate the learning of the second list. Ebbinghaus learned 16-syllable lists by the method of complete presentation. Learning was to a criterion of one perfect repetition. He then rearranged the syllables so that the originally adjacent items were separated by either 1, 2, 3, or 7 intermediate syllables. Finally, after twenty-four hours, Ebbinghaus learned the new lists. The scheme of rearrangement can be made clear if one thinks of the 16 items of the first list as running, in order, from 1 to 16, and then examines the sequence of numbers given below for two derived lists. When one syllable is skipped, every syllable but one (no. 2) has next to it a syllable which had been one position distant from it in the first list. When two syllables are skipped, every item but two has next to it one which had been two positions removed in the original list. In this arrangement there are irregular gaps from 16 to 2 and from 14 to 3. The number of these gaps increases with the number of syllables skipped.[1]

Skipping one syllable: 1, 3, 5, 7, 9, 11, 13, 15, 2, 4, 6, 8, 10, 12, 14, 16.

Skipping two syllables: 1, 4, 7, 10, 13, 16, 2, 5, 8, 11, 14, 3, 6, 9, 12, 15.

It is assumed that, if remote associations have been formed, the derived lists will be learned in a shorter time than will a control list which, as constructed by Ebbinghaus, consisted of the first and last syllables in their original positions and of the intervening 14 in a chance order. The combined results of two of his experiments (Table I) show a saving in time for each of the derived lists, but a saving which decreases in amount as the degree of remoteness (number of skipped items) increases. Relearning the list in exactly the same order (0 syllables skipped) yields the greatest saving, while the chance arrangement yields none. These results are taken to mean that associations of zero degree of remoteness are strongest,

[1] The scheme for Ebbinghaus' derived groups will be found on page 97 of his monograph. In the rearrangement for seven skipped items he introduced items of this degree of remoteness from different, but equally familiar, lists.

but that remote forward associations exist with a strength which is a decreasing function of their degree of remoteness. The decreases in saving with increasing degrees of remoteness are not sufficiently reliable to make the conclusion sure, although the regularity of the tendency creates a presumption in its favor.

TABLE I

TIME SPENT IN LEARNING ORIGINAL AND DERIVED LISTS

(After Ebbinghaus, *Memory*, p. 106)

No. of Intermediate Syllables Skipped in the Formation of the Derived Lists	Time for Learning the Original Lists (Sec.)	Time for Learning the Derived Lists (Sec.)	Saving in Per Cent of Original Learning Time
0	1266	844	33.3
1	1275	1138	10.8
2	1260	1171	7.0
3	1260	1186	5.8
7	1268	1227	3.3
Chance	1261	1255	0.5

The interpretation of Ebbinghaus' results is made difficult by the fact that he used the method of complete presentation. This method gives the learner an opportunity to look backward and forward over the list, and hence, to form "adjacent associations" between non-adjacent items. This inadequacy is not inherent in the method of derived lists since it may be corrected by presenting the items, one at a time, on a memory drum. The use of the method of complete presentation by Ebbinghaus, who had practiced this form of learning under rigid experimental conditions, may not have influenced his results. Untrained subjects, however, might be expected to look back and forth over the list, even without intending to do so, and in this manner perceive 1 and 3, or 5 and 9 together and to associate them directly.[2]

[2] Two experiments have been omitted from this discussion because the methods employed seem to the writers to make their results indeterminate. Cason (1926) found practically no evidence for remote associations, but degree of learning in his experiment was very high. This condition is known to decrease remote associative tendencies. Hall (1928) found increased speed of

Müller and Schumann (1894) have suggested that the irregular gaps in Ebbinghaus' derived lists might have produced the relationship between strength of remote associations and degree of remoteness. It has already been noted that the number of these gaps increases with the number of skipped items. Müller and Schumann contend that the more of these gaps there are, the smaller will be the effect of remote associations on the learning of the derived list. One may ask, however, if this suggestion does not assume that strength of remote associations is inversely related to degree of remoteness; otherwise, the irregular gaps, with their greater degree of remoteness, would not have the effect of reducing the saving.

The method of derived lists, however, does have certain limitations which should be made explicit. The results it yields are unanalyzed and permit only the conclusion that some remote associations must have been formed. In other words, it does not tell us the specific items which have been remotely associated. The method also permits unknown amounts of associative interference. That under certain conditions associative interference may be an important variable has been demonstrated by Irion (1946) who found that the interpolated learning of a derived chance-order list produces large amounts of retroactive inhibition. This interference cannot serve except to reduce remote association effects. The positive evidence for remote association is not a function of such interference, although remote associations, themselves, may be a function of *positive* transfer of training.

The Association Method

The basic feature of this method of studying remote associations is that, after the material has been learned, some or all of the items are presented singly and the subject is instructed to respond with the first response available to him. In comparison with the method of derived lists, this has an advantage in that it elicits individual, specific associations whose degree of remoteness is known, and does

learning of her derived lists, but her results are difficult to interpret because of the way in which her derived lists were constructed.

not leave their existence to be inferred from differences between the rates of learning original and derived lists.

An association method was employed first by Wohlgemuth (1913), who sought only for immediately adjacent forward and backward associations. The method was extended by McGeoch (1936) and Raskin and Cook (1937) to include the study of remote associations of any degree of remoteness. More recently, this method has been employed by Wilson (1943, 1949). During testing by this method, the learned items are presented one at a time in a random order to the subject, who is instructed to respond as rapidly as possible with his first association to that item. Usually the instructions to, and the set of, the subject keep these associations within the materials which were learned, but occasionally an "extraneous" association is given. The first and last items in the series are omitted from the test, but the subject can give the last item as a response in the forward direction and the first item as a response in the backward direction. In McGeoch's experiment, the task consisted of the learning of a list of ten two-syllables adjectives. Thus, during the test, there were seven possible degrees of remote forward association plus the adjacent forward association. There were also nine possible degrees of backward association. We shall deal here only with the forward associations.

The data of Table II, corroborated by those of three other experiments, show a large number of recalls of words separated from the stimulus word by one or more intervening items. There is, thus, no doubt that by this method considerable numbers of remote associations have been found. They are recalled with latencies (association reaction times), which are consistently lower, although by small amounts, than the latencies of the adjacent associations. Raskin and Cook (1937) have obtained frequencies of remote associations at different degrees of remoteness which are similar to those obtained by McGeoch.[3]

[3] One difficulty with determining the relationship between frequency of remote associations and degree of remoteness lies in the fact that there are fewer possible associations of a high degree of remoteness than of a low degree of remoteness. When the second item in a 10-item list is the stimulus, the highest degree of remoteness possible is 7 (the number of skipped terms

Results obtained by the association method provide a more direct picture of the specific remote associations formed, are freer than the method of derived lists from the possible masking influences of associative inhibition, and leave no doubt that forward remote associations occur in relatively large numbers.

TABLE II

FORWARD AND BACKWARD ASSOCIATIONS IN LISTS OF ADJECTIVES

(From McGeoch, *Amer. J. Psychol.*, 1936, 48, pp. 225-231)

Degree of Remoteness	Forward Associations Total	Forward Associations Mean Time (Sec.)	Backward Associations Total	Backward Associations Mean Time (Sec.)
0	211	1.76±.12	147	1.29±.08
1	68	1.46±.09	52	1.46±.09
2	44	1.45±.11	50	1.51±.12
3	39	1.09±.09	31	1.30±.11
4	32	1.55±.19	27	1.17±.10
5	38	1.23±.15	16	1.17±.14
6	18	1.31±.14	20	1.18±.13
7	12	1.20±.14	13	1.43±.26
8			3	1.59±.57

The Method of Anticipatory Errors

During the practice of a serial act, subjects often make responses which would be correct at a later point in the series, but which, at the point made, are errors. These are anticipatory errors, the occurrence of which we have already noted. The analysis of anticipatory

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between it and the tenth item). When the third item is the stimulus, the highest degree of remoteness possible is 6, and so on. A simple correction might be made for these differences in opportunity to give the different degrees of remote association by multiplying each frequency by a fraction which has the total number of stimuli which could have associations of zero degree in the numerator and the number of stimuli which could have responses of a given positive degree of remoteness in the denominator. Thus, if 8 words could have associations of zero degree and only 4 words could have associations of 4 degrees of remoteness, the obtained number of associations of that degree would be multiplied by 8/4. Raskin and Cook (1937) have applied this correction to their data, with the result that frequency decreases from zero to the third degree of remoteness and increases thereafter. More recently, Bugelski (1950) has employed this same general type of correction factor.

errors provides another method for the study of remote forward association.

The illustrative data of Table III are the numbers of anticipatory errors made by groups of subjects in learning letter mazes where the correct "path" or sequence of letters was to be found by trial and error on the keyboard of a typewriter.[4] The table shows large numbers of anticipations and a definite tendency for their number to decrease as the degree of remoteness increases. Lumley also found that, as learning progressed, the anticipation of more remote letters became less and the anticipation of less remote ones became greater. Under these conditions, therefore, remote forward associations decrease with increasing degree of remoteness, and this inverse relationship becomes more pronounced as practice goes on.

TABLE III

AVERAGE NUMBER OF ERRORS MADE ON THREE LETTER MAZES, ANTICIPATING THE FIRST, SECOND, THIRD, ETC., LETTERS AHEAD

(From Lumley, *J. exp. Psychol.*, 1932, 15, p. 200)

	Number of Letters Ahead						
Maze	1	2	3	4	5	6	7
I	66.7	49.8	43.3	32.9	31.3	31.1	24.7
II	15.1	11.4	9.8	6.8	7.1	8.4	6.4
III	28.6	17.6	22.8	15.5	17.4	9.7	12.4

In an experiment by Mitchell (1934) anticipatory errors in learning three-place numbers fall off sharply from 59.8 per cent at one skipped term to 9.6 per cent at two skipped terms, and slowly thereafter. Mitchell's data, however, fail to give evidence supporting Lumley's conclusion that the more remote associations decrease and the nearer ones increase as learning progresses. In a more recent experiment, Bugelski (1950) has noted the tendency for the number

[4] In Lumley's (1932) experiment, from which Table III has been taken, a typewriter was wired to give an auditory signal when the "right" key was struck, and the subject was to press keys by trial and error until the right one was found. As a result of practice, the subject eventually found a series of letters each of which, when chosen in the proper order, would give the auditory signal. References to other reports of anticipatory errors will be found in Lumley's papers (1931, 1932).

of remote associations to be a negatively accelerated decreasing function of the degree of remoteness.

The anticipatory error method has certain advantages over the two other methods. The remote associations, whatever their degree of remoteness, are overt and specific, which is not the case under the method of derived lists, though it is by the association method. Remote associations may be observed as a function of amount of practice in a single group of subjects. This cannot be done with the method of derived lists, and can be done with the association method only if an association test is made after each practice trial or after selected blocks of trials. If tests are made in this way, and practice continued thereafter, learning during the remainder of practice may be a function of the tests. If practice is not continued, a separate learning situation must be arranged for each individual test. The method of anticipatory errors is not, however, without its limitations. Anticipatory errors are relatively infrequent in many learning situations, so that a great number of subjects must be run in order to observe any considerable number of them. Furthermore, this method can yield no information concerning the number of remote associations in material which has been learned to a high criterion of perfection, since on the criterial trials there can be no anticipatory errors and, during continued repetition after the criterial trials, errors are extremely infrequent. Each of the methods for studying remote associations has its limitations and its advantages. Adequate knowledge concerning remote associations can be obtained only by the use of all of them.

Conditions of Which Remote Associations Are a Function

(A) *Time interval between original learning and the test for remote associations.* Using the method of derived lists, Lepley (1934) has made a systematic study of the influence of time interval upon the occurrence of remote associations. He found a statistically significant influence of remote associations only after an interval of 30 minutes. When 10 minutes, 1, 3, 6 or 24 hours intervened between the learning of the original and the derived lists, no significant

influence of remote association was found. This, of course, is in disagreement with the findings of Ebbinghaus' (1885) experiments, wherein remote association effects were probably demonstrated after an interval of 24 hours.

Using the association method, Wilson (1943) has investigated this relationship. His findings, which are in disagreement with those of Lepley, are summarized in Table IV. Although Wilson was work-

TABLE IV

MEAN NUMBER OF REMOTE ASSOCIATIONS AS A FUNCTION OF TIME SINCE LEARNING

(From Wilson, *J. exp. Psychol.*, 1943, 33, p. 44)

Time between Learning and the Test for Remote Associations	Mean Forward Remote Associations at Recall	Mean Backward Remote Associations at Recall	Total
0.5 Min.	4.54	3.69	8.23
2.0 Min.	4.54	3.65	8.19
5.0 Min.	4.27	3.73	8.00
20.0 Min.	3.83	3.29	7.12

ing with shorter time intervals than was Lepley, the tendency revealed by his data is clear. Remote forward associations tend to decrease throughout his range of retention intervals. This decrease, however, is very slight and does not approach statistical significance. Wilson's results cannot be considered, however, as supporting Lepley's conclusion that remote associations increase for some time following the conclusion of practice. That this difference in results is not due to differences in amount of learning, or to differences in the massing of practice during original learning is suggested by later results obtained by Wilson (1949). In this experiment, a slight decrease in number of remote associations was obtained as the retention interval was increased from zero minutes to twenty minutes. Again, this decrease was not statistically significant, but the obtained gradient had almost exactly the same slope as was obtained in Wilson's earlier experiment. This consistency of results lends a

certain credibility to the proposition that the number of remote associations, as measured by the association method, tends to decrease with increasing time from original learning. This conclusion is by no means sure, however. Whether such a generalization holds for remote associations as measured by other methods, or whether it is an artifact of the association method is even less certain. In view of the discrepancy between the results obtained by Lepley and by Wilson, this latter possibility must be regarded as a subject for future systematic investigation.

(B) *Degree of learning.* Ebbinghaus repeated lists of 16 syllables either 16, 32, or 64 times and measured the percentage saved by the method of derived lists (one skipped item) after an interval of 24 hours. Percentage of saving, the indicator of remote association, decreased as number of repetitions (and, presumably, degree of learning) increased. (Table V.) It will be noticed that, with increasing frequency of repetition, the saving on the original lists increased rapidly, while the saving on the derived lists increased more slowly. Results of similar meaning have been published by Lepley (1934) in a continuation of the experiment which has already been mentioned. With a constant time interval of 30 minutes between original learning and relearning (or learning of the derived list), the derived lists showed a saving when learning had been to one perfect trial, but not when it had been to six perfect trials. Using the association method, Wilson (1949) found a significant decrease in number of remote associations as degree of learning increased.

TABLE V

THE RELATIONSHIP BETWEEN FREQUENCY OF REPETITION AND DEGREE OF REMOTE ASSOCIATION

(From Ebbinghaus, *Memory*, p. 116)

Number of Repetitions	Seconds Saved in Relearning Original Lists	Seconds Saved in Learning Derived Lists	Per Cent Saving for Derived Lists Is of That for Original
16	192	100	52
32	407	149	37
64	816	161	20

The influence of this variable may also be studied during the course of practice by means of the method of anticipatory errors. This method yields a continuous measure of remote associations, in so far as they express themselves in this type of error. The data obtained by Lumley (1932) from a variety of materials reveal an inverse relation between degree of remoteness and stage of practice, that is, there is a decrease in the number of errors of the higher degrees of remoteness and a relative increase in those of the lower degrees. This is shown in the ratios of Table VI. These ratios were obtained by dividing the sum of the values for the lower degrees

TABLE VI

RATIOS OF THE FAR ANTICIPATIONS TO THE NEAR ANTICIPATIONS IN FOUR SUCCESSIVE PERIODS OF PRACTICE

(From Lumley, *J. exp. Psychol.*, 1932, 15, p. 342)

Learning Activity or Material	No. of Subjects	Period of Practice I	II	III	IV
Letter maze	38	2.225	.577	.459	.316
Two-place numbers	25	.906	.411	.329	.157
Paper maze	25	.644	.426	.196	.106
Foot maze (1)	9	1.007	.376	.373	.169
Foot maze (2)	32	.736	.268	.173	.120

(as 1 and 2) into the sum of those for the highest degrees (as 4 and 5). The ratios become progressively smaller as practice goes from the first period through the three roughly equal divisions of the remaining trials. Although it is not supported by Mitchell's (1934) findings concerning the memorization of numbers, Hull (1935a) corroborates this finding in nonsense syllable learning. Anticipatory errors of all degrees must finally disappear, of course, if a criterion of one or more perfect trials is to be reached.

When remote associations are studied by the methods of association and derived lists, they are found to decrease with increasing frequency of repetition (or degrees of learning). It is to be noted that it is logically necessary for remote associations to increase during the initial stages of practice. This is because remote associations are, themselves, learned responses and hence, must be acquired

during the early stages of practice. Only Mitchell (1934) has reported this relationship. In her experiment, the number of anticipatory errors first increased and then decreased as learning progressed.

(C) *Other conditions.* Several other potential determining conditions of remote associations have been investigated. Wilson (1949) failed to demonstrate a significant relationship between frequency of remote associations and the degree of distribution of practice during original learning. Mitchell (1934) reports that the faster learners made more anticipatory errors than did the slower ones, and it may be that speed of learning and other *individual characteristics of the learner* may be important. Hull (1935a) has shown that five grains of caffeine citrate clearly increases the number of anticipatory errors. In both the caffeine condition and the control condition, the number of intrusions is greater in the middle of the list. A number of other probable conditions should be studied, such as the instructions to the learner and his resultant set, the mode of presentation of the material, the character of the material learned, and the occurrence of positive and negative intra-list transfer of training.

Associations in the Backward Direction

When a series of responses has been learned in a given temporal sequence or in a "forward" order, can these responses also be made in the reverse or "backward" order? If they can, is this backward association confined to adjacent terms, or is it also between spatially separated (remote) terms? In a series of terms, A, B, C, D, E, and F, learned in that sequence, is F connected with E and also with D or C or B or A? These questions cannot be answered by pointing to one's relative inability to repeat the alphabet backward with any fluency or to recite a poem in reverse order of the words or lines. The alphabet has been overlearned and dealt with in too many contexts to make conclusions from its repetition significant, and to recite a poem backward is to reduce the meaningful to a meaningless sequence of words which has no important relation to the

original. The problem can be attacked only in newly learned series of relatively homogeneous responses. For this purpose the three methods already discussed in this chapter, or variants of them, have been among the chief ones employed.

The Method of Derived Lists

From his work on remote forward associations Ebbinghaus went on to a brief study of backward connections. This time the derived lists were composed of the syllables in the original list either in a reversed order throughout or in a reversed order but skipping alternate syllables on the same principle as the skipping of one term in the forward direction. The learning of the lists in reversed sequence after twenty-four hours was 12.4 per cent faster than the learning of the original lists had been, while the learning of the derived lists with alternate syllables skipped was only 5 per cent faster. From this, Ebbinghaus concluded that backward associations had been formed during the original learning, not only between adjacent syllables but also, though to a smaller extent, between syllables separated from each other by one term. Here, as in his experiments on remote forward association, the specific associations formed are not known, but the formation of at least some associations in the direction specified is inferred from the faster learning of the derived list. The influence of backward association is somewhat less, under these conditions, than the influence of forward associations of a comparable degree of remoteness.

Critics were quick to object that the method of complete presentation used by Ebbinghaus gave the subject opportunity to look back and forth in the list and thus to form associations in the reverse order of intended presentation. He might, for example, look backward from Syllable 8 to Syllable 3, react to them as if adjacent, and connect them. This same objection we encountered in connection with the study of remote forward associations. It is, of course, peculiar to the method of complete presentation. Garrett and Hartman (1926) have put this criticism to experimental test by

comparing the amounts of backward association obtained by the method of derived lists when learning had been by complete presentation and when it had been at the rapid rate of 0.8 second per syllable. In one set of derived lists, the syllables were reversed. In another, the order of successive pairs was reversed. Both sets gave evidence of backward association, but no significant difference appeared between the amounts of backward association under the two different methods of presentation. In spite of this finding, Garrett and Hartman believe that backward associations are improbable. This belief rests upon their further belief that their subjects had formed adjacent connections between spatially remote items, even with the rapid exposure. Under their special conditions of measuring recall, this may have been possible. Their results support Ebbinghaus' conclusion that backward associations are formed, but leave open the problem of whether or not they represent adjacent associations formed as an artifact of the method of learning.

The work of Bunch and Lund (1932) on backward association in animal learning furnishes further evidence on this problem. They found that six mechanically guided trials in one direction through a maze exerted a small facilitating influence upon learning in the backward direction, but only when trials were used as the measure. The implications of the results of Dorcus (1932) are similar, though the controls necessary for adequate determination of the influence of backward association are not all present. Insofar as white rats have formed backward associations under the conditions of these experiments, we may assume direct practice in the backward direction to have been minimal.

The Association Method

In an early experiment by this method, Wohlgemuth (1913) found that subjects, left free to give any one of the two items adjoining the relevant item, gave associations which were in the proportion 1.3 in the forward direction to 1.0 in the backward direction in the learning of diagrams. In the learning of syllables, the ratio was 1.7 to 1.0. When the subjects were instructed to recall in one

direction or the other, recall in the instructed direction was dominant, but there were some recalls in the opposite direction. When, for example, the instructions were to recall the syllables in the forward direction, the ratio of forward to backward associations was 3. 4 to 1.0, but it is important to note that there were still recalls in the backward direction.

The relatively large numbers of backward associations, both at the adjacent position and at degrees of remoteness 1 through 7, obtained by McGeoch (1936), have already been summarized in Table II. These backward associations were recalled with mean latencies similar to those for forward associations. The ratio of adjacent backward to adjacent forward associations is 1.0 to 1.43, but the corresponding ratio for the total numbers of remote associations is 1.0 to 1.18. Clear evidence of backward association has also been reported by Wilson (1943, 1949) and by Raskin and Cook (1937). The data of Wohlgemuth, McGeoch, Wilson, and Raskin and Cook leave no doubt that remote backward associations are formed under conditions where simultaneous stimulation by the associated items is not possible.

The Method of Perseverative Errors

In learning lists of verbal items, subjects sometimes give a response which is correct earlier in the list but wrong at the point where it is given. This type of error, we have noted, is called a perseverative error because it represents the carrying forward of an earlier response. Perseverative errors have been much less studied than anticipatory errors, and where they have been reported, their numbers have been small (Bugelski, 1950). They do occur, however, and may be regarded as representing backward associations, since presentation of a stimulus word elicits as a response a word which came before it in the list. In serial learning, conditions do not favor the appearance of perseverative errors. The subject has recently seen the earlier items and has probably responded correctly to many of them, making it less probable that he should give one of them again. Patten (1938) has shown that perseverative

errors tend to be more frequent under massed practice than under distributed practice.[5]

The Significance of Remote Forward and of Backward Associations

We have seen that, during practice in the forward direction, associations are formed not only between adjacent items but also between spatially separated ones. By means of these remote associations a series becomes interconnected and woven into a whole which is much more complexly organized than serial succession, alone, would indicate. This positive fact is of high importance to an understanding of mental organization. The effect of remote associations, however, may be negative as well as positive, in that they may interfere with the formation of unbroken serial responses. That they may do this is clear from their appearance as anticipatory and perseverative errors which must be suppressed if learning is to be completed. The learning and retention of any series is a composite function of the positive and negative effects of remote association. Remote associations may enter into the explanation of distributed practice, the reminiscence phenomenon, transfer of training, and many other aspects of learning and retention.

In spite of less extensive work upon backward associations, they have been found to exist under certain conditions between both adjacent and remote items. They are probably weaker and less frequently formed than forward connections. This is to be expected on the ground that series learned in the forward direction are easier to reproduce in that direction. If forward and backward associations were formed in equal degrees, series would function with equal readiness in either direction, and might become a battleground of associative interference. It is also to be expected that associations

[5] The problem of backward association arose in connection with the study of verbal learning. With the introduction of conditioning techniques, however, the problem has been given a new method of attack and a somewhat different meaning. Conditioning is said to be in the forward direction when the conditioned stimulus is presented before the unconditioned stimulus and backward when the unconditioned stimulus is given first. This problem has already been discussed in Chapter III.

between adjacent terms in the forward direction should be stronger than between remote items, else smooth response in a given serial order would be impossible.

When backward associations, either adjacent or remote, are formed, they contribute a more intricate and subtle organization of the terms than forward association of immediately adjacent items alone provides. The interwoven whole of the learned series is still more complexly connected by them, and they enter equally, in principle, with remote forward associations into the picture of intraserial learning. They probably act, also, as determiners of other phenomena.

Hypotheses To Account for Remote Forward and Backward Association

The existence of remote forward and backward associations which may be formed without direct practice raises the question of the way in which these connections are made. They are formed across an interval of time greater than that between adjacent items, an interval filled by response to other items. They are formed in some other way than by the direct response to the two terms together, and, when made in the backward direction, they are in the reverse temporal order of the subject's response to them. The hypotheses offered to account for these phenomena will be summarized.

(A) *The Lepley hypothesis.* Lepley (1934) has formulated an hypothesis to account for remote forward associations in terms of conditioned-response concepts. He assumes that, in addition to adjacent associations, termed simultaneous conditioned responses, there are remote excitatory tendencies, suppressed for appropriate intervals by inhibitory tendencies, and having the character of trace-conditioned responses. Thus, each item except the last two in a list may be the conditioned stimulus to each item ahead of it, but the remote excitatory tendencies are held in check by inhibition of delay. Trace-conditioned responses have anticipatory characteristics, however, which may result in anticipatory errors of the kind already described.

From this hypothesis, Lepley had deduced that, by using the method of derived lists constructed with skipped alternate items and the same exposure interval as during learning, a test immediately after learning would reveal no saving, while one after a longer time would reveal saving. The reasoning is that the items in the original list appear in the order 1, 2, 3, ... *N*, but those in the derived list have the order 1, 3, 5, ... *N*. Thus, the interval between 1 and 3 is only half as long as it was in the original list. The inhibitory delay phase, established during original learning, is inappropriate to the interval at the test. With time, however, the inhibitory tendencies should dissipate and remote associations should appear. We have already seen that he found significant evidence of remote association only after thirty minutes, a fact which he interprets in support of his hypothesis.[6]

The Lepley hypothesis rests upon an analogy between conditioned response learning and serial verbal learning which is ingenious, but which presents difficulties. It may be noted that items in a serial list do not lend themselves readily to classification as conditioned and unconditioned. Even if one grants that the analogy to conditioning is valid, however, one cannot overlook the fact that trace-conditioned responses are formed slowly and with difficulty in the conditioning of single responses, while remote associations may be elicited by the association method after as few as five trials and appear as anticipatory errors still earlier in practice.[7] There seems, moreover, to be no correspondence between the latencies

[6] It will be recalled that Wilson (1943, 1949) obtained results which tended to indicate that remote associations decreased as the time interval increased. These results, alone, do not constitute an embarrassment for Lepley's hypothesis since they were obtained by the association method where the time between the stimulus and the response is not controlled. However, taken in conjunction with McGeoch's (1936) findings that remote associations obtained by the association method have, if anything, shorter latencies than do adjacent associations, these results suggest that the Lepley hypothesis may not be entirely adequate.

[7] It is recognized, of course, that the occurrence of an anticipatory error represents the relative failure rather than the relative success of establishing a trace-conditioned response. That is, poorly established trace-conditioned responses tend to occur immediately upon the presentation of the conditioned stimulus. Well-established responses of this kind, however, do not occur until a definite time period following presentation of the conditioned stimulus.

which remote associations, interpreted as trace-conditioned responses, might be expected to have, and those found by the method of association. Both the method of anticipatory errors and the method of association reveal remote associations soon after learning under conditions of delay which do not fit the Lepley hypothesis. As it stands, the hypothesis is interesting and suggestive of research, but requires extensive experimental testing before the extent of its interpretive potency can be known. If it is to include backward associations, this, too, remains to be worked out.

(B) *The Hull hypothesis.* Hull (1935b, 1940) has greatly extended the Lepley hypothesis and has made it cover other phenomena, such as serial position effects to which we shall come later. These extensions can best be described in the setting of other specific problems. The most important new feature for the present problem is his postulate (1940) that each item in a verbal series leaves a stimulus trace in the organism which continues with diminishing strength throughout the remainder of a trial. The items which come later are simultaneous with this trace and, through it, become connected with remote items. By this means, associations between spatially remote items, which are not responded to by the subject as if adjacent, are still adjacent associations, in the sense that the influence of the presented item on the organism becomes directly connected with the stimulus trace of prior items. This view is a more precise specification of the trace-conditioned response which is basic to the Lepley hypothesis.

Hull's theory is reasonable and will account for a large number of facts within a single theoretical framework. It leaves some questions unanswered, of which one is the relation between speed of association and degree of remoteness. The stimulus trace is assumed to diminish at a negatively accelerated rate, and the more remote the connected terms, the weaker the trace of the first one would be when the second is presented. This should bring about a weaker association with a correspondingly longer latency. Actually, the association method reveals no clear relation between degree of remoteness and latency. This, however, is an embarrassment faced by any theory which holds strength of association to diminish with

degree of remoteness while simultaneously asserting latency to be a measure of associative strength.

(C) *The Guthrie hypothesis.* Guthrie (1935) holds that remote associations are possible because the stimulus item is still being responded to when the remote response item is presented. The continued or recurrent response to the first item goes on, after the manner of Hull's stimulus trace, while the intervening items are being presented. Stimuli arising from these responses may become conditioned to later items in the series. This continued response is not necessarily reportable by the subject, nor is it a specific surrogate of the initial stimulus, but may be a series of movements set going by this stimulus. Association between spatially remote items is still adjacent association, however, because it is between the continuing effects of one term and the immediate presentation of another. Hull and Guthrie agree on this basic point, and on the assumption that something, set going by one item, continues while later items are presented. Hull assumes an unspecified trace in the organism. Guthrie assumes a continued response of some kind.

Neither Hull nor Guthrie has worked out a systematic account of backward associations in theoretical terms. Guthrie's theory is the more apparently open to direct experimental attack by means of a search for the continuing response, but actually this search is extremely difficult. No matter where one fails to find the response, unless one could have a complete measure of the response of every muscle fiber in the organism, a defender of the theory could reply that one had not looked in the right place. This is not to say that the theory is inadequate, but that it is immensely difficult to attack. If research leads to positive results, the elusiveness of the theory may vanish.

(D) *A mediate association hypothesis.* It is possible, although no one has defended this view, that remote associations might be mediated by a connection between two or more items of a list and a common third item. The theory of the existence of mediate association is an old one which has generated more discussion than straightforward experimental study. Peters (1935) has shown that mediate associations do occur, if they are defined as the occurrence

of two items together in recall "because they have previously been associated with a common item." Likewise, one item may arouse another when the common item is perceptually or ideationally present at the time of recall.

The concept may be illustrated by one of Peters' experiments in which subjects learned a list of twenty paired words and letters and then learned a list in which digits were the initial items and the same letters as before were the second items. Later the subjects were presented with the words and asked to give the first number that came to mind. In this and other experiments mediate associations appeared with a frequency which varied greatly from subject to subject and which tended to vary directly with the meaningfulness of the material. Their numbers are not great, but their occurrences is undoubted.

It is conceivable that spatially remote items in a list might be associated through the connection of each with a third or mediating item. This mediating factor might be anything whatever in the environmental and experimental context, or it might be some common factor in the subject's own reactions, illustrated by an association between the items and the common verbalization, "this list." One difficulty with this theory is that associations with items outside the material being learned might be expected to be too weak and easily inhibited to mediate the number of remote associations actually found. Another difficulty lies in the fact that the theory does not account for the decrease in remote associations with increasing degrees of remoteness. This latter difficulty might be solved by hypothesizing mediate associations of the second and third degree, but these, in turn, have never been independently demonstrated. Since no theory is, as yet, entirely acceptable, the possibility of mediate association is mentioned as one which may account for at least some of the remote associations. In principle, the mediate association theory can account for backward associations, since, so far as is now known, mediate associations are not confined to one direction.

(E) *A generalization hypothesis.* A generalization theory of remote associations would hold that anticipatory and perseverative

errors occur because of stimulus generalization. According to this view, when any stimulus-response relationship in a series gains strength, an increment may be expected in the tendency for similar stimuli also to elicit that response. The presentation of a second stimulus item, similar to the first, might evoke a tendency for the learner to respond incorrectly with the response to the first item. Although this theory has not been formally advanced, the possibilities of using generalization as an explanatory concept in verbal learning theory have been explored by Gibson (1940). Such a theory also receives inferential support by the results obtained by McGourty (1940). The theory does not account in any obvious way for the decreasing relationship which has been found to exist between number of remote associations and degree of remoteness. On the other hand, explanation of backward association presents no problem to it. One deduction which should follow from such a theory is that more remote associations should be found in the learning of a homogeneous (all items similar) series than in the learning of a heterogeneous series.

(F) *Acquaintance.* In his book, *Association Theory To-day,* Robinson (1932) has suggested the factor of acquaintance as one which may have been influential in producing what Ebbinghaus and others have taken to be remote associations. By acquaintance is meant practice at reading or reciting the syllables without regard to the temporal order in which they are learned in the test list. Waters (1939), however, has shown that varying amounts of prior reading and reciting of syllables do not significantly facilitate their learning in a new order. The numbers of repetitions devoted to acquainting the subjects with the syllables in this experiment were less than those usually required for learning a list, but Waters' results raise doubt that the amount of acquaintance provided by learning a list would greatly affect the rate of learning it in a rearranged order. Robinson's hypothesis, of course, was designed to explain data obtained by the method of derived lists, and was stated by Robinson before any considerable amount of data had been collected by means of other methods. At the present time, this hypothesis must be regarded as unpromising.

(G) *The initial reproductive tendency hypothesis.* The theories of Lepley, Hull, and Guthrie have been formulated primarily to deal with remote forward associations and are incomplete to the extent that they do not as thoroughly include associations in the backward direction. The mediate association hypothesis and the generalization hypothesis, on the other hand, account for both remote forward and backward associations. A possibility exists, of course, that forward and backward remote associations occur under different conditions and that two explanations, rather than one, are required. A special hypothesis to account for backward associations has been suggested by Meyer (1939). This hypothesis states that backward associations may often be instances of the *initial reproductive tendency* described by Müller and Pilzecker (1900). By this tendency they meant the fact that the presentation of one, and particularly the last, member of a learned series of items tends to elicit the initial item followed by the others in the sequence. The word "Hesperus" is likely to elicit "The Wreck of the Hesperus," the final word of the known series eliciting the first. Meyer presented short lists, each line or item of which was composed of three nonsense syllables to be learned by the subjects, with the accent on various syllables under different conditions. After an interval of ten minutes, one syllable from each triplet was presented, with the instruction to speak whatever syllables came to mind. The subjects showed a clear tendency to give the first syllable of the triplet, regardless of other conditions, thereby corroborating Müller and Pilzecker. This would ordinarily be called backward association, but Meyer insists that it is not. If it were, then backward associations in his results would be stronger than forward associations, because there is more backward than forward reproduction; and remote backward would be stronger than direct backward, because when the third syllable is presented, the first one is aroused more often than the second. These possibilities Meyer rejects.

Instead of calling the results illustrations of an initial reproductive tendency, Meyer regards the forward temporal order of learning as an aspect of the complex or whole which was learned, and the results as examples of a tendency to reproduce the whole in the

order in which it was experienced. On this view, associations are not backward in the sense usually meant, but are only apparently so as a result of this tendency to reproduce the whole complex. His empirical results are clear. It is unknown whether the tendency to reproduce the whole, beginning with the initial item, is more than a habit of beginning at the beginning, elicited in a given case by the remote connection between each following item and the first one. This interpretation was framed in the context of short series of three syllables each. It has not been offered in explanation of the large number of remote associations found by the association method or of the much less frequent ones found under the perseverative-error method.

Contiguity and Remote Association

The hypotheses of Lepley, Hull, and Guthrie agree in assuming that some process initiated by the items in a presented series continues, without overt or reportable effort on the part of the subject, until the presentation of later items. The process established by each earlier item is contiguous with that set up by each later one, and associations between them are by contiguity. This emphasis on contiguity flows from the difficulty of conceiving a connection between temporally separated events which are not connected by physical processes of some kind. The mediate association hypothesis and the generalization hypothesis, however, do not account for remote associations in terms of the contiguous presentation of the associated items. Rather, these hypotheses make use of other contiguously learned responses in their explanations. The mediate association theory appeals to the demonstrable fact that two associations made with a common item may result in one stimulus eliciting a response with which it has never been directly associated. The generalization hypothesis appeals to the facts of stimulus generalization and transfer of training for its explanation. In neither type of theory, however, is it assumed that associations may be formed between items which are separated in time without the mediation of some kind of physical process.

LEARNING AS A FUNCTION OF SERIAL POSITION

Many of the conventional learning materials are composed of readily discriminable parts, many of which are formally equal. Verbal lists are composed of separate items, mazes are composed of sections of paths separated from each other by turns, and some rational problems are a series of steps or parts. In such cases, learning of an item or part may be plotted as a function of its serial position. The earlier experimenters on this problem stated it in terms of primacy and recency, as if the question were concerned only with the relative ease of learning the first and the last items in a series. The problem is more general than this, however. The general question is concerned with the form of the relationship between the ordinal position of an item in a series and the rate of learning of that item.

Characteristic Serial Position Curves

The anticipation method of learning verbal lists is admirably fitted to yield clean-cut data on serial position effects because it assures the exposure of each item for a constant time and because it provides a trial-by-trial record of the subject's behavior on each item. The curves published by Ward (1937) for lists of 12 nonsense syllables are typical of serial position curves obtained in verbal lists. (Figure 10.) These curves show the effects of serial position at different stages of practice from the trial on which only 3 items are correctly anticipated through the trials on which 5, 7, 9, and 11 are correct. The high points occur at the ends of the list. The curves then dip down from either end to a low point near the middle of the list to give the bow-shaped curve which the usual measurements with the anticipation method show. The extent of the central dip is large early in practice, but decreases as practice proceeds. The first positions have an advantage over the final ones during the early trials, but this difference diminishes toward zero as complete learning is approached. The curve for the criterial trials must necessarily become a straight, horizontal line. It will be noticed that,

until learning is complete, the major portion of the dip in the curve is toward the right of the center of the series, giving a curve which is not symmetrical.

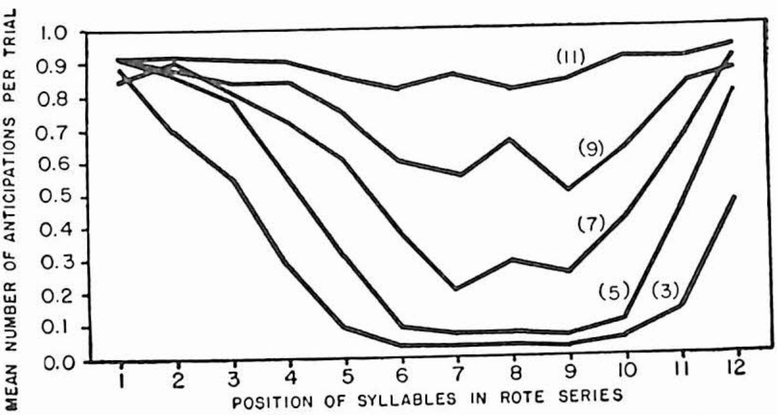

FIG. 10. MEAN NUMBER OF CORRECT ANTICIPATIONS AT EACH POINT IN THE ROTE SERIES FOR FIVE LEVELS OF MASTERY

(From Ward, *Psychol. Monogr.*, 1937, 49, No. 220, p. 35)

The individual curves are each marked with the level of mastery represented. Curves are given for levels of mastery of 3, 5, 7, 9, and 11 correct anticipations, respectively, in a single learning trial. The graphs are based on the data secured from 12 different lists learned by each of 12 subjects.

Curves of this general form appear under a large number of conditions. They are not confined to verbal lists, but may appear, though with less regularity of form, in some maze learning and in some rational learning problems. Warden (1924) has shown, for example, that in the learning of a multiple-U stylus maze the average number of cul-de-sac entrances into the first three alleys is 11.2, into the middle four is 23.3, and into the final three is 16.9. The first and last parts of the maze are learned with fewer errors than is the middle, and the first part with fewer than the last. Cul-de-sac entrances are less cleanly isolated units than are verbal items, but, even so, the constancy of the relation between serial position and rate of learning in different groups of subjects is high. The rank-difference correlation between Warden's records for mean number of entrances and data obtained by Peters and McGeoch (1935) is 0.987.

Measurements of the learning of a number of mazes agree in revealing at least a rough primacy-finality superiority, with accompanying greater difficulty of the intermediate choice-points. When one considers the behavior complexity elicited by a maze, no high stability of relation between serial position and rate of learning is to be expected from maze to maze. Unless different sections of a maze are homogeneous, regularity of relationship with serial position is not to be expected. Serial position curves obtained in measurements of animal learning will not be considered here, and the major emphasis will be upon results obtained with verbal lists.[8]

Serial Position Curves of Oscillations at the Threshold of Recall

In learning by the anticipation method, subjects frequently anticipate any given item one or more times and then fail to anticipate it for one or more trials before the next correct response. Hull (1935) has called such a sequence of successes and failures *an oscillatory cycle at the threshold of recall.*[9] In the following record for a single item, in which a minus sign indicates a failure to anticipate and a plus sign indicates a correct anticipation, there are three oscillation cycles, one oscillation being counted each time one or more consecutive plusses are followed by one or more consecutive minuses.

$$- - - + - - + - - + + - + + +$$

[8] The literature concerning the investigation of analogous problems in animal maze learning is extremely rich. Order of blind alley elimination is a function of a great many variables. Some of these are reviewed by Buel (1935). Other important papers include those by Hull (1931, 1932, 1934a,b), Yoshioka (1929), Spence (1932), Spence and Shipley (1934), and Grice (1942). One outstanding difference in the form of the serial position curve obtained in animal studies and those reported in this chapter is the general finding subsumed under the concept of the goal gradient—that there is a backward elimination of errors from the goal. Hull (1948) has shown that this may be a function of terminal reinforcement since, when reinforcement is given after each correct choice, the serial position curve much more nearly approaches the type obtained in verbal learning experiments.

[9] This oscillatory cycle is an empirical event as used here and should not be confused with the postulated, oscillating inhibitory potential, $_SO_R$, which plays a role in Hull's (1943, 1950) later theoretical systems.

Hovland (1938) and Shipley (1939) have independently shown that the number of oscillations between the first success and the last failure resembles the serial position curves which depict only number of correct responses (or of failures). This means that there is, at the ends of the list, a smaller spread between the first success and the last failure than in the middle of the list. Once a subject has correctly anticipated the early and late items he is less likely to fail to anticipate them (forget them) on the following trials than he is the once-anticipated intermediate items.

The number of oscillatory cycles in the learning of any item is, thus, a function of the serial position of the item. It may be that this number provides another measure of the amount of interference to which an item is subjected.

Conditions of Which Serial Positions Curves Are a Function

The serial position curves which have been described are characteristic of those found under a large number of conditions. They should not, however, be regarded as absolutes. The frequent appearance of the bow-shaped relationship means merely that, as verbal lists and mazes are often constructed and learned, the conditions which determine this relationship are relatively stable. Conditions have been found which disrupt and change this relationship, and conditions could probably be devised to yield any desired form of serial position curve.

(A) *Direction of effort.* Subjects are usually instructed to devote an equal amount of effort to each item in a list, and this instruction may be one of the conditions which favors the bow-shaped serial position curve. Kreuger (1932) has measured the serial position effects when three groups of subjects learn lists of 12 pairs of logically unrelated nouns, each group working under a different instruction. One group was told only to learn as rapidly as possible; one was told to learn the first 3 and the last 3 pairs first; a third was instructed to learn the middle 6 pairs first. Learning was tested after each presentation by giving the stimulus words in a different order

from that of presentation. When no specific instruction is given, the first 3 and the last 3 positions yield much more rapid learning than the middle 6, but, when effort is directed first to the middle 6, they are learned more rapidly than the items on the ends. Instruction to learn the end positions first increases, somewhat, the already high values at these positions. This unsurprising result indicates that the conditions which determine the shape of the serial position curve under ordinary circumstances of learning are not influential enough to overcome differences in distribution of effort put forth by the subject.

(B) *Uncontrolled order of recall.* Investigators have occasionally presented a list of verbal items once or oftener to their subjects and have then permitted free recall in any order. Under this procedure, subjects often recall the last items first, with a resulting serial position curve like that of Figure 11. This method introduces unequal time intervals and varying amounts of interpolated activity between the presentation and recall of items in different positions, and the form of the relation between serial position and the measure of learning is a function of the peculiarities of this method. In a study which compared the serial position curves obtained with five different methods of presenting nonsense syllables and measuring learning, Raffel (1936) obtained five different serial position curves, varying from the usual bow-shaped curve obtained under the anticipation method to a skewed curve like that of Welch and Burnett (1924) shown in Figure 11. That a method which gives equal formal opportunity to every position is to be preferred in measuring serial position effects should not obscure the fact that the relationship which is obtained may be a function of the method used to obtain it.

(C) *Massed versus distributed practice.* Lists of 12 nonsense syllables are learned to a criterion more rapidly under distributed practice in which a rest interval of two minutes and six seconds is introduced between trials than under massed practice in which the trials follow each other with only six-second intervals between them. The difference between the two conditions of practice is much greater at the central positions of a series than at the ends,

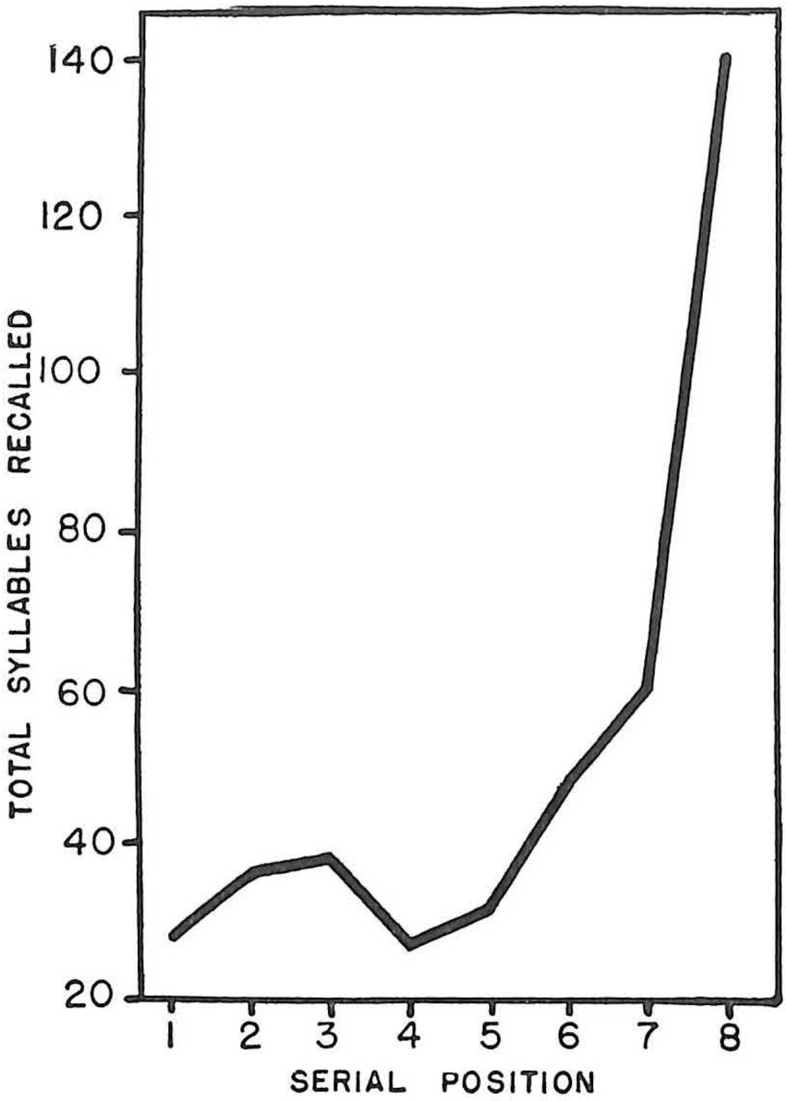

FIG. 11. SERIAL POSITION CURVE OBTAINED BY METHOD OF UNCONTROLLED SEQUENCE OF ATTEMPTED RECALL

(From Welch and Burnett, *Amer. J. Psychol.*, 1924, 35, p. 399)

as Figure 12 reveals. Where the serial position curves thus far reproduced have been curves of positive performance, those in Figure 12 are for mean number of failures. Results obtained under the anticipation method may be shown with equal clarity in either way. These curves of Hovland's (1938b) show that the double gradient

of serial position effect is steeper under massed than under distributed practice, values at the ends being nearly coincident under the two conditions. In another study, Hovland (1938a) has found that this difference increases as the lists increase in length from 8 to 14 items.

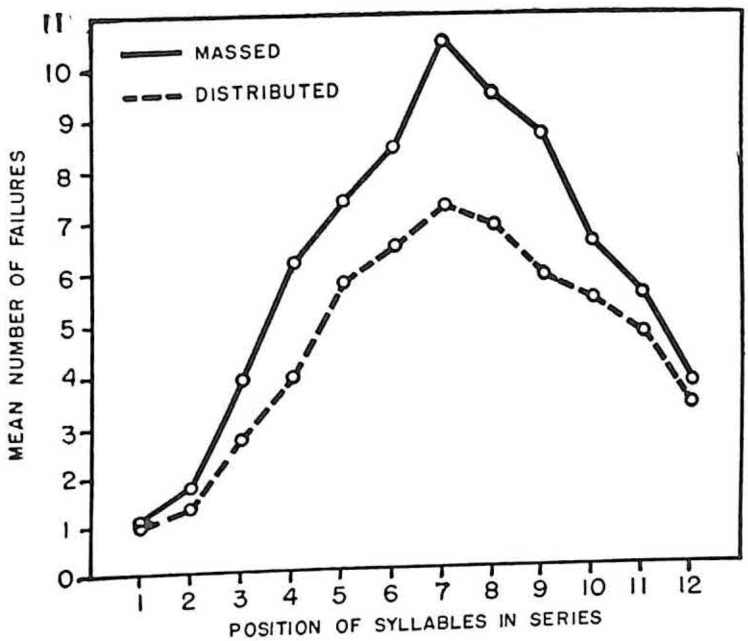

FIG. 12. COMPOSITE CURVES SHOWING MEAN NUMBER OF FAILURES AT VARIOUS SYLLABLE POSITIONS INVOLVED IN LEARNING TO MASTERY BY MASSED AND BY DISTRIBUTED PRACTICE WITH TWO-SECOND RATE OF PRESENTATION

(From Hovland, *J. exp. Psychol.*, 1938, 23, p. 178)

Each point is based upon the mean of 64 learning scores. Failures are computed beginning with the first trial on which the subjects attempt recall, i.e., the second rotation of the drum.

(D) *Speed of syllable presentation.* Hovland (1938) has also shown that the steepness of the serial position gradients is a function of the rate of presentation. When the presentation time of each syllable in a 12-syllable list is increased from two seconds to four seconds, the mean trials required to attain a criterion of 7 syllables correct out of 12 decreases from 6.05 to 3.28. The greatest reduction in the number of errors comes in the central positions, partic-

ularly at the customary point of greatest difficulty, that is, just beyond the center of the list. (Figure 13.) Smaller, but similar changes take place in the number of oscillations at the threshold of recall.

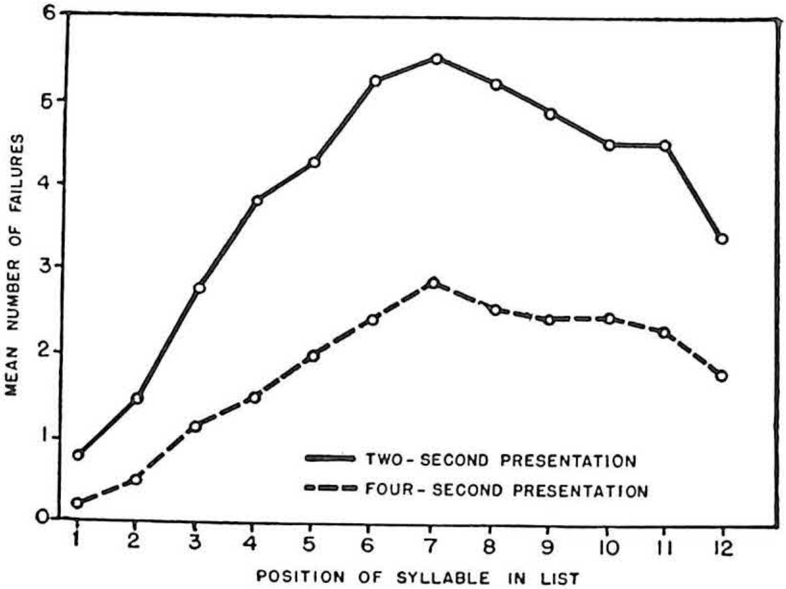

FIG. 13. COMPOSITE GRAPHS SHOWING NUMBER OF FAILURES MADE AT EACH SYLLABLE POSITION DURING LEARNING TO A CRITERION OF 7 SYLLABLES CORRECT WITH TWO-SECOND AND FOUR-SECOND RATES OF PRESENTATION

(From Hovland, *J. exp. Psychol.*, 1938, 22, p. 341)

Each point is based upon the mean of 128 determinations. Failures are computed beginning with the first trial on which the subjects attempt recall, i.e., the second rotation of the drum.

(E) *Amount of practice at learning.* It has long been known that practice at learning successive lists of verbal material brings a considerable increase in rate of learning. The graph in Figure 14 shows that, with this increase in rate, there may also come a flattening of the serial position curve, an increased relative advantage of the first position, and an increasing tendency to learn from the first of the series. The data from Lepley's (1934) work are for the first and second six lists learned, but approximately twice as much practice had been obtained during this period upon lists not mentioned here. The tendency to make fewer errors early in the list has been

shown to have increased from the first list onward in a continuous manner.

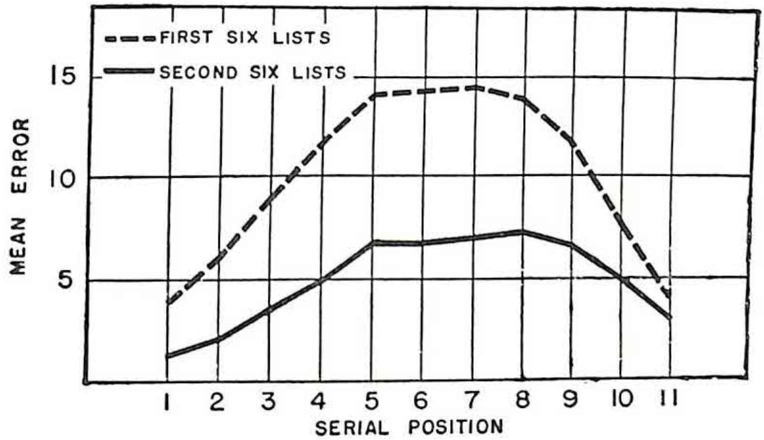

FIG. 14. SERIAL POSITION CURVES AT TWO DIFFERENT STAGES OF PRACTICE (From Lepley, *Psychol. Monogr.*, 1934, 46, No. 205, p. 42)

(F) *Length of list.* The characteristics of the serial position curve vary as a function of the length of the series being learned. Robinson and Brown (1926) and Hovland (1940) have obtained data which indicate that the point of maximum difficulty in the series changes systematically as length of list is increased. The data presented in Table VII illustrate this shift. It will be noted that as length of list increases, the *relative* distance from the beginning (or center) of the list to the point of maximum difficulty decreases.

TABLE VII

THE RELATIONSHIP BETWEEN LENGTH OF LIST AND RELATIVE POSITION OF THE POINT OF MAXIMUM DIFFICULTY AS MEASURED IN TERMS OF MEAN NUMBER OF FAILURES DURING PRACTICE

(From Hovland, *J. exp. Psychol.*, 1940, 27, pp. 271-284)

A Length of List	B Point of Maximum Difficulty	Per Cent B is of A
8	6	75%
11	7	63%
14	8	57%

(G) *Other conditions.* The conditions discussed have been sufficient to demonstrate that, even with homogeneous materials practiced under carefully controlled conditions, the detailed characteristics of the bow-shaped serial position curve vary with conditions, of which distribution of practice, rate of presentation, length of list, and amount of prior practice are examples. The introduction of variations in the direction of effort or of uncontrolled order of recall will alter the curve still further. The variations which may be introduced are numerous. It is the implication of Von Restorff's (1933) work on another problem, for example, that heterogeneity of the material, such as is represented by substituting a two-place number for one of the syllables in a list of nonsense syllables, may sharply alter the curve at that position in favor of the isolated item. Pillsbury and Raush (1943) have shown, however, that as the number of heterogeneous items is increased, this isolation effect regularly decreases.

Lepley (1934) has found that a sampling of Grade VII children shows a less marked primacy effect than a sampling from Grade XI, although both groups exhibited an increasing primacy effect as practice with similar lists continued.

Not all conditions which affect rate of learning cause variations in the relation with serial position. Mitchell (1933) finds that use of continuous presentation of lists of three-place numbers from the first trial to the criterial trial does not markedly change the serial position curve from that obtained by Robinson and Brown (1926) who introduced the customary short temporal gap between trials.

Recently, Malmo and Amsel (1948) have studied serial position effects in clinical patients suffering from severe anxiety. Their results indicate that the shape of the serial position curve may be altered by what they term *anxiety produced interference.* This interference shows itself during the early part of the series and causes a decrease in the amount of skewness which is usually found in the serial position curve. Under similar conditions, six bilateral frontal gyrectomy cases were studied. The results from this group indicate that organic damage of this type leads to an increase in the typical serial position effect. Malmo and Amsel hold that anxiety-

produced interference results from interference by irrelevant responses arising out of the patient's anxiety state while the organic damage results in an increased susceptibility to intra-serial interference.

The suggestion has frequently been made that the rapid rate of learning at the initial position or positions may be a function of rehearsal, a result of the subject's thinking back over the earlier items during practice on the later ones. This may be possible, but it is not supported by the studies of the serial position curve under conditions of slow and fast presentation rates. Here, it will be remembered, a greater relative advantage accrues to the initial position under fast presentation, a condition which allows less opportunity for rehearsal. There is also the possibility that the faster learning of the early and late items may be a function of rehearsal during the brief intervals between trials. During this interval, the learner might think of the last few items presented to him and then of the few he is to be asked to recall when the next trial begins. Against this hypothesis stands the result obtained by Mitchell (1933) who did not use an interval between trials. Also against it is the fact that trained subjects do not report such rehearsal, yet show bow-shaped curves, that serial position effects are obtained on the first trial of completely presented material recalled immediately (as by the memory span method), and that inter-trial distribution of practice decreases rather than increases this phenomenon. When one considers, also, the susceptibility of serial position curves to variation with other conditions with which there is no reason to expect rehearsal to vary similarly, one must conclude that rehearsal is not a major determiner of the curves obtained.

Hypotheses Concerning Serial Position Effects

(A) *Hull's deduction of serial position effects.* Of all the theories which have been constructed to explain serial position effects, the one proposed by Hull (1940) is, by far, the most comprehensive and complex. In an earlier formulation (1935b), Hull had explained serial position effects in terms of the Lepley hypothesis.

This theory, which is illustrated by the diagram in Figure 15, involved the assumption that each stimulus item in the series becomes connected to each later response by connections analogous to trace-conditioned responses. Thus, during practice of the middle items, there are present, simultaneously, inhibitions connected with these trace-conditioned responses. This inhibition is considered to be analogous to the inhibition of delay obtained in delayed and

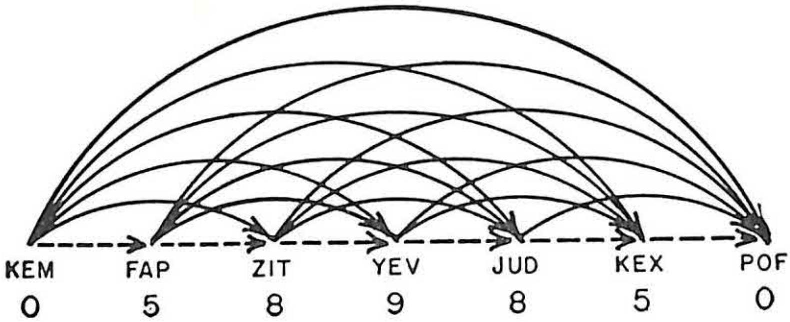

FIG. 15. DIAGRAMMATIC REPRESENTATION OF BOTH THE IMMEDIATE AND REMOTE FORWARD EXCITATORY TENDENCIES ASSUMED TO BE OPERATIVE IN ROTE SERIES

(From Hull, *Psychol. Rev.*, 1935, 42, p. 502)

The straight broken arrows represent immediate excitatory tendencies, and the curved solid arrows represent remote excitatory tendencies. The number of remote excitatory tendencies spanning the several syllables is given beneath each.

trace-conditioned response experiments (cf. Chapter III). Responses in the intermediate positions are, thus, subject to an inhibitory influence, the amount of this influence being dependent upon the number of trace-conditioned responses which span the item in question. One difficulty with this hypothesis is that it places the point of maximum difficulty in the center of the list. In actuality, the point of maximum difficulty appears to fall beyond the center of the list and appears, furthermore, to vary in position with certain of the conditions of learning. This inadequacy, among others, led to the publication of the much more extensive *Mathematico-deductive theory of rote learning* (1940) by Hull and his collaborators.[10] This work represents one of the most ambitious attempts to construct learning theory and is far too lengthy and complex to be discussed

[10] C. I. Hovland, R. T. Ross, M. Hall, D. T. Perkins, and F. B. Fitch.

in detail here. In general, the theory deals with three classes of rote-learning phenomena: serial position effects, the course of acquisition of rote-learned responses, and retention (including reminiscence). The serial position curve is dealt with in terms of a modification of the Lepley hypothesis which yields an excellent quantitative fit to much serial position data. The postulate system predicts the skewness of the serial position curve and some of the variations in the position of the point of maximum difficulty. The effects of distribution of practice and the effects of a single interpolated rest period are also predicted with considerable accuracy. Some deductions concerning minor phenomena have not yet been subjected to experimental test. Furthermore, an occasional deduction may be found which contradicts the findings of a particular experiment. However, when one considers the wide variability of human learning data and the relative lack of standardization of rote-learning experimental situations, small discrepancies of this sort are not particularly surprising.

One point concerning this type of theoretical formulation should be emphasized. The *formal properties* of the system should be sharply distinguished from the *names* which Hull gives to the processes under consideration. The formal properties are those which are defined by experimental (stimulus) events and the postulated interactions between these intervening variables, so defined. For instance, Hull postulates the existence of "inhibitory potential." This concept he formally defines in terms of experimental variables. Hull goes on, however, to identify (in prose) this concept with the Pavlovian concept of internal inhibition. This identification is unnecessary in the sense that definition, in terms of experimental operations, has already been accomplished. It becomes useless, therefore, to argue whether or not the concept of internal inhibition may be applied to rote-learning phenomena or whether inhibitory potential (in this system) behaves exactly as does the inhibition of delay. The only important consideration is the internal consistency of the system and the precision of prediction made on the basis of the stimulus-inferred intervening variables and the postulated relationships between them.

A final word should be said in appraisal of Hull's theory. The value of this work lies not only in its precision in predicting serial position effects, but also in the range of the phenomena the system covers. Since, within a single postulate system, Hull is able to predict a wide range of other phenomena, the theory possesses a comprehensiveness which greatly increases its value. That many of the postulates will almost certainly have to be modified in the future only points to the need for continuing theoretical work of a precise, quantitative nature.

(B) *Explanation in terms of competing responses.* Hull's explanation of the phenomena of serial position is in terms of inhibition exercised by position-spanning remote associations. These inhibitions, as we have seen, are presumably identifiable with the phenomenon of inhibition of delay and, as such, have the property of decreasing any excitatory tendencies which might be elicited during their existence. Another possibility of inhibition exists in the failure of a particular response to occur owing to the simultaneous elicitation of another, incompatible, response tendency. During the learning of a rote-series, if two responses have a tendency to be elicited by a single stimulus, usually both of these responses cannot be made. Either the correct response can be made or an error can occur, depending upon which one of the response tendencies prevails. Still more common, in all probability, is the mutual blocking of the two response tendencies so that no response at all occurs. The competition between two or more mutually incompatible responses offers, then, a potential explanation of difficulty in learning various items in a rote-series. In order to explain serial position data, however, two additional explanations are required. These are: (1) how and why does a particular stimulus come to acquire tendencies to evoke more than one response, and (2) assuming that this form of inhibition does exist, why does it distribute itself differentially over the series? The latter it must do if serial position curve data are to be explained. A number of possibilities exist with respect to each of these questions.

As to the first question, it will be recognized that we are asking for an explanation of remote associations. The fact of remote as-

sociation creates an almost conclusive presumption that various stimuli in a series acquire multiple response tendencies. We have already reviewed various hypotheses which attempt to account for remote associations. They include: (1) the setting up of trace-conditioned responses, whether in terms of a perseverating neural trace (Hull) or in terms of a perseveration of stimulus-producing activity (Guthrie), (2) the formation of mediate associations, and (3) the occurrence of remote associations by means of stimulus generalization.

The second question poses a more difficult problem. One possible explanation lies in the assumption that strength of remote associations is a negatively accelerated, decreasing function of degree of remoteness combined with the assumption that backward associations are weaker than forward ones of a comparable degree of remoteness. In view of the data which have been presented earlier in this chapter, both of these assumptions would appear to be reasonable ones. If the proper functions are postulated, and if it is assumed that the strength of the adjacent forward associations is decreased by interference to an extent which is proportional to the combined strengths of all remote associations (including backward associations), a serial position curve may be predicted which bears close resemblance to those which are actually obtained. A recent theoretical-experimental paper by Bugelski (1950) explores this possibility as far as remote forward associations are concerned.

A closely related point of view was first proposed in 1920 by Woodworth and Poffenberger and apparently independently in 1928 by Foucault.[11] On this view, there occur within a series two interference effects of the same kind as those which are known to occur between series. These will be described first as inter-serial phenomena and then applied to learning within a single series. One is *proactive inhibition,* which is the interference effect of learning a first list (or other activity) upon the learning of a second. The other is *retroactive inhibition,* which is the interference effect

[11] These two points of view may be reduced to a single hypothesis if certain assumptions are made regarding the nature of proactive and retroactive inhibition.

of a second list upon the retention of its predecessor. Within a series, each item may have a proactive effect on all following items, and each one after the first may have an inhibitory effect (retroactive inhibition) upon all preceding ones. In the learning of a series, the initial term is completely free from proactive inhibition, and the early items are relatively freer from it than are the later ones. In a like manner, the final item is entirely free from retroactive inhibition, and the later items are freer from it than the earlier ones. The early and late items in a list will have an advantage, therefore, over the intermediate items because, in the latter case, the two forms of inhibitory action are both in operation.

In support of the hypothesis that the serial position curve may be a function of proactive and retroactive inhibition, Foucault's (1928) data may be offered. He has presented serial position values for the immediate recalls of lists of 3, 4, 5, 6, and 7 words by children aged ten to fourteen. If we consider the influence of retroactive inhibition to be measured by the number of times the first word in a list is omitted or forgotten, and the influence of proactive inhibition to be measured by the number of times the last word is forgotten, the results (Table VIII) are what one would expect if each one of

TABLE VIII

NUMBER OF FIRST AND LAST ITEMS FORGOTTEN IN LISTS OF DIFFERENT LENGTHS

(From Foucault, *Année Psychol.*, 1928, 29, p. 100)

	Number of Words in the List				
	3	4	5	6	7
First items forgotten	2	12	26	37	47
Last items forgotten	2	9	39	34	33

the inhibitory effects were a function of the number of potentially effective items. The number forgotten at the first position increases steadily as the list increases in length. The number forgotten at the last position fails to increase without inversion, but this does not invalidate the general hypothesis.

If the two forms of inhibition work additively, the fourth word in a 7-word list should suffer the sum of the forgettings of the first

and last items in a series of four words, because there are 3 words that may interfere proactively and 3 that may interfere retroactively in each case. The sum of the words forgotten at the first and last positions in a list of four words is 21, but the total number of times the fourth word in a 7-word list is forgotten is 81. The combined action of proactive and retroactive inhibition must be assumed to be greater than the simple sum of the influences of the 3 antecedent and the 3 consequent words. Foucault accounts for this by assuming that the connections of succeeding words are weakened by proactive inhibition and are, thus, less able to resist retroactive inhibition from subsequent words.

One difficulty with this view is that, if the influences of proactive and retroactive inhibition are assumed to be equal, then the point of maximum difficulty should fall in the middle of the list. In order to predict a properly skewed curve, it is necessary to assume that proactive inhibition is stronger than retroactive inhibition. This assumption would be difficult to make on the basis of Foucault's data or on the basis of the findings regarding proactive and retroactive inhibition as these operate inter-serially. One way to resolve this problem is to assume that the later items in the list are more susceptible to the effects of retroactive inhibition because of the weakening effects of proactive inhibition.

This theory may be attacked directly by discovering whether the form of the serial position curve can be made to vary by changing certain similarities among the component parts of a list. Amount of inter-list proactive and retroactive inhibition is a function of the degree of similarity between the two lists, and, if the present theory is valid, amount of intra-list interference should be a function of the similarities between different sections of the list. Using specified amounts of similarity between the Witmer consonant syllables in the two ends of certain lists and among the middle four syllables in other lists, McGourty (1940) has produced certain variations in the form of the serial position curve which the theory would predict. Lists constructed in a way to distribute randomly the formal similarity which is inevitable when the items are constructed solely from consonants gave the usual bow-shaped curve (A in Figure 16).

When, however, the first three and the last three items are formally similar (*i.e.*, have common letters, as MXK and BKX) both within each group of three and between the two groups of three, Curve B in Figure 16 is obtained. The mean number of correct anticipations per trial at each of these six positions is considerably lower than at the corresponding positions in the control (random formal similarity) group, and at the same time, the values at the intermediate four

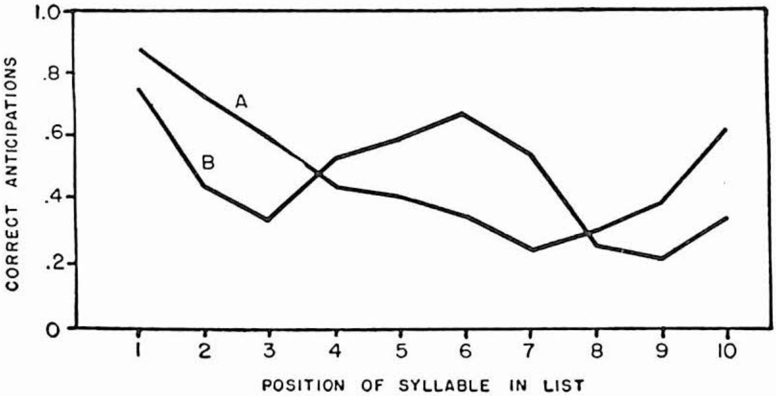

FIG. 16. MEAN NUMBERS OF CORRECT ANTICIPATIONS PER TRIAL (A) WITH FORMAL SIMILARITY RANDOMLY DISTRIBUTED AND (B) WITH HIGH FORMAL SIMILARITY AMONG AND BETWEEN THE FIRST 3 AND LAST 3 ITEMS IN A 10-ITEM LIST OF CONSONANT SYLLABLES

(From McGourty, Univ. Iowa, 1940)

positions are clearly increased. Not only has the introduction of formal similarity decreased the amount of learning of the similar items, but it has yielded a more rapid learning of the intermediate items. This confirmation of the deduction from the interference hypothesis tends, of course, to support that interpretation.[12]

The competition of response point of view is not as rigorously worked out as is the Hullian theory, and deductions made from it

[12] It goes without saying that this finding also supports an hypothesis based on remote associations occurring as a result of stimulus generalization. McGourty's results appear to be clearly in support of an interference rather than an inhibition of delay point of view. Hull (1940) makes no assumptions about the development of inhibition as a function of item similarity. On the other hand, assumptions of this kind could be introduced without changing the fundamental structure of Hull's system, a fact that Hull, himself, apparently recognized before the publication of McGourty's results, as a result of work done by Gibson (1938).

are much less precise in character. It is difficult, therefore, to compare these points of view. It is, however, entirely possible that the two theories may eventually be brought together in the form of a single hypothesis.

(C) *Serial position curves as a function of two types of learning.* Recently, Ribback and Underwood (1950) have advanced an ingenious hypothesis to account for serial position data. Starting from the frequently observed fact that subjects tend to learn a serial list from the ends (working toward the middle), they note the possibility of two distinct learning processess. The first of these involves the usual formation of adjacent forward associations. The second type of learning, which operates at the posterior portion of the list, involves the formation of "backward associations" of a rather special kind. That is, the subject learns that item 9 is the stimulus for item 10 and then must reverse this process (giving 10 for 9) during actual recitation. These two forms of learning, Ribback and Underwood term A and B learning, respectively. If A learning occurs more readily than B learning, a possible explanation of the skewed serial position curve exists. In an experiment designed to test the relative rates of A and B learning, two groups of subjects were first required to learn a list of 6 pairs of nonsense syllables presented in a random order and learned by the method of paired-associates. Each group then learned a derived list, which was composed of the original pair of syllables plus a third syllable. This third syllable was placed, in one group, to serve as a response to the original pair presented as a stimulus. In the other group, the third item was placed as a stimulus for the elicitation of the originally learned pair. This may be schematized as follows:

Original list	QEL (S_1)	FIP (R_1)	
Derived list (A learning)	QEL (S_1)	FIP (R_1, S_2)	MYD (R_2)
Derived list (B learning)	MYD (S_2)	QEL (R_2, S_1)	FIP (R_1)

It will be seen that the learning of the first derived list is an example of A learning while the learning of the second derived list is an

example of B learning. Since the results of this experiment demonstrate the faster learning of the first type of derived list (A learning), the hypothesis proposed by Ribback and Underwood is supported. Although not enough data exist for a thoroughgoing evaluation of this hypothesis, it is to be hoped that more experimental work will be conducted with respect to this theory.

(D) *Set and association with position.* It has been suggested (Woodworth and Poffenberger, 1920) that *association with position* is another basic condition of serial position curves.[13] The first and last items are the ones most likely to be associated with their ordinal numbers or some other symbolic equivalent of serial position. To some extent the other early and late items share in this association with position, but the middle items are much less likely to become associated in this way because the intermediate serial positions are less clearly identified and discriminated during practice. The serial positions, coming in an order already known to the subject, presumably act by providing a ready-made mnemonic system into which he fits the new items.

Working hand in hand with this association between position and item may be the influence of a set toward the first and last items, a set which may spread somewhat to give the double gradient so often found. Kreuger's results have already demonstrated the effectiveness of set as a condition of the serial position effect. This hypothesis, in terms of set and association with position, lacks the generality of the Hullian hypotheses or the competition of response theory, and has led to no great amount of experimental study. It has long been known, however, that subjects may and do use serial order as a mnemonic aid, so that the hypothesis is not without foundation. It can give a good account of the primacy-finality effect and of the slower learning of the intermediate items. On the other hand, it has no ready answer for the fact that the slowest learning is past the center of the list. In general, then, this must be regarded as the least promising of the present-day theories of the serial position phenomena.

[13] For more recent evidence of association with position and its role in interserial interference, cf. Melton and Irwin (1940).

REFERENCES

Buel, J. Differential errors in animal mazes. *Psychol. Bull.*, 1935, 32, 67-99.

Bugelski, B. R. A remote association explanation of the relative difficulty of learning nonsense syllables in a serial list. *J. exp. Psychol.*, 1950, 40, 336-348.

Bunch, M. E., and Lund, W. R. An experiment on backward associations in animal learning. *J. comp. Psychol.*, 1932, 13, 143-156.

Cason, H. Specific serial learning; a study of remote forward association. *J. exp. Psychol.*, 1926, 9, 299-324.

Dorcus, M. D. The establishing of backward associations in forward learning of the maze by albino rats. *J. comp. Psychol.*, 1932, 13, 11-18.

Ebbinghaus, H. *Das Gedächtnis: Untersuchungen zur experimentellen Psychologie.* Leipzig: Duncker and Humblot, 1885. (Trans. as *Memory: a contribution to experimental psychology* by H. A. Ruger and C. E. Bussenius. New York: Teachers College, Columbia Univ., 1913.)

Foucault, M. Les inhibitions internes de fixation. *Année Psychol.*, 1928, 29, 92-112.

Garrett, H. E., and Hartman, G. W. An experiment on backward association in learning. *Amer. J. Psychol.*, 1926, 37, 241-246.

Gibson, E. J. A systematic application of the concepts of generalization and differentiation to verbal learning. New Haven: Ph.D. Thesis deposited in Yale University Library, 1938.

Gibson, E. J. A systematic application of the concepts of generalization and differentiation to verbal learning. *Psychol. Rev.*, 1940, 47, 196-229.

Grice, G. R. An experimental study of the gradient of reinforcement in maze learning. *J. exp. Psychol.*, 1942, 30, 475-489.

Guthrie, E. R. *The psychology of learning.* New York: Harper, 1935.

Hall, M. E. Remote associative tendencies in serial learning. *J. exp. Psychol.*, 1928, 11, 65-76.

Hovland, C. I. Experimental studies in rote-learning theory. II. Reminiscence with varying speeds of syllable presentation. *J. exp. Psychol.*, 1938a, 22, 338-353.

Hovland, C. I. Experimental studies in rote-learning theory. III. Distribution of practice with varying speeds of syllable presentation. *J. exp. Psychol.*, 1938b, 23, 172-190.

Hovland, C. I. Experimental studies in rote-learning theory. VII. Distribution of practice with varying lengths of list. *J. exp. Psychol.*, 1940, 27, 271-284.

Hull, C. L. Knowledge and purpose as habit mechanisms. *Psychol. Rev.*, 1930, 37, 511-525.

Hull, C. L. Goal attraction and directing ideas conceived as habit phenomena. *Psychol. Rev.*, 1931, 38, 487-506.

Hull, C. L. The goal gradient hypothesis and maze learning. *Psychol. Rev.*, 1932, 39, 25-43.

Hull, C. L. The concept of the habit-family hierarchy and maze learning. *Psychol. Rev.*, 1934a, 41, 33-54; 134-152.

Hull, C. L. The rat's speed of locomotion gradient in the approach to food. *J. comp. Psychol.*, 1934b, 17, 393-422.

Hull, C. L. The influence of caffeine and other factors on certain phenomena of rote learning. *J. gen. Psychol.*, 1935a, 13, 249-273.

Hull, C. L. The conflicting psychologies of learning—a way out. *Psychol. Rev.*, 1935b, 42, 491-516.

Hull, C. L. Mind, mechanism, and adaptive behavior. *Psychol. Rev.*, 1937, 44, 1-32.

Hull, C. L., *et al. Mathematico-deductive theory of rote learning: a study in scientific methodology*. New Haven: Yale Univ. Press, 1940.

Hull, C. L. *Principles of behavior*. New York: D. Appleton-Century, 1943.

Hull, C. L. Reactively heterogeneous compound trial-and-error learning with distributed trials and serial reinforcement. *J. exp. Psychol.*, 1948, 38, 17-28.

Hull, C. L. Behavior postulates and corollaries—1949. *Psychol. Rev.*, 1950, 57, 173-180.

Irion, A. L. Retroactive inhibition as a function of the relative serial positions of the original and interpolated items. *J. exp. Psychol.*, 1946, 36, 262-270.

Kreuger, W. C. F. Learning during directed attention. *J. exp. Psychol.*, 1932, 15, 517-527.

Lepley, W. M. Serial reactions considered as conditioned reactions. *Psychol. Monogr.*, 1934, 46, No. 205.

Lumley, F. H. An investigation of responses made in learning a multiple choice maze. *Psychol. Monogr.*, 1931, 42, No. 189.

Lumley, F. H. Anticipation of correct responses as a source of error in the learning of serial responses. *J. exp. Psychol.*, 1932, 15, 195-205.

Lumsdaine, A. A. Conditioned eyelid responses as mediating generalized finger reactions. *Psychol. Bull.*, 1939, 36, 650.

Malmo, R. B., and Amsel, A. Anxiety-produced interference in serial rote learning with observations on rote learning after partial lobectomy. *J. exp. Psychol.*, 1948, 38, 440-454.

McGeoch, J. A. The direction and extent of intra-serial associations at recall. *Amer. J. Psychol.*, 1936, 48, 221-245.

McGourty, M. C. Serial position effects in learning as a function of interfering associations. Iowa City: M. A. Thesis deposited in State University of Iowa Library, 1940.

Melton, A. W., and Irwin, J. McQ. The influence of degree of interpolated learning on retroactive inhibition and the overt transfer of specific responses. *Amer. J. Psychol.*, 1940, 53, 173-203.

Meyer, G. Temporal organization and the initial reproductive tendency. *J. Psychol.*, 1939, 7, 269-282.

Mitchell, M. B. The effect of serial position in the continuous memorization of numbers. *Amer. J. Psychol.*, 1933, 45, 493-494.

Mitchell, M. B. Anticipatory place-skipping tendencies in the memorization of numbers. *Amer. J. Psychol.*, 1934, 46, 80-91.

Müller, G. E., and Pilzecker, A. Experimentelle Beiträge zur Lehre vom Gedächtnis. *Z. Psychol.*, 1900, Ergbd. 1.

Müller, G. E., and Schumann, F. Experimentelle Beiträge zur Untersuchung des Gedächtnisses. *Z. Psychol.*, 1894, 6, 81-190.

Patten, E. F. The influence of distribution of repetitions on certain rote learning phenomena. *J. Psychol.*, 1938, 5, 359-374.

Peters, H. N. Mediate association. *J. exp. Psychol.*, 1935, 18, 20-48.

Peters, H. N., and McGeoch, J. A. The relationship between time spent in the culs-de-sac of a stylus maze and speed of elimination. *J. exp. Psychol.*, 1935, 18, 414-430.

Pillsbury, W. B., and Raush, H. L. An extension of the Köhler-Restorff inhibition phenomenon. *Amer. J. Psychol.*, 1943, 53, 293-298.

Raffel, G. Two determinants of the effect of primacy. *Amer. J. Psychol.*, 1936, 48, 654-657.

Raskin, E., and Cook, S. W. The strength and direction of associations formed in the learning of nonsense syllables. *J. exp. Psychol.*, 1937, 20, 381-395.

Ribback, A., and Underwood, B. J. An empirical explanation of the skewness of the bowed serial position curve. *J. exp. Psychol.*, 1950, 40, 329-335.

Restorff, H. v. Über die Wirkung von Bereichsbildungen im Spurenfeld. (Analyse von Vorgängen im Spurenfeld.) *Psychol. Forsch.*, 1933, 18, 299-342.

Robinson, E. S. *Association theory to-day: an essay in systematic psychology.* New York: Century, 1932.

Robinson, E. S., and Brown, M. A. Effect of serial position upon memorization. *Amer. J. Psychol.*, 1926, 37, 538-552.

Shipley, W. C. An apparent transfer of conditioning. *J. gen. Psychol.*, 1933, 8, 382-391.

Shipley, W. C. Indirect conditioning. *J. gen. Psychol.*, 1935, 12, 337-357.

Shipley, W. C. The effect of a short rest pause on retention in rote series of different lengths. *J. gen. Psychol.*, 1939, 21, 99-117.

Spence, K. W. The order of eliminating blinds in maze learning by the rat. *J. comp. Psychol.*, 1932, 14, 9-27.

Spence, K. W., and Shipley, W. C. The factors determining the difficulty of blind alleys in maze learning by the white rat. *J. comp. Psychol.*, 1934, 17, 423-436.

Ward, L. B. Reminiscence and rote learning. *Psychol. Monogr.*, 1937, 49, No. 220.

Warden, C. J. Primacy and recency as factors in cul-de-sac elimination in a stylus maze. *J. exp. Psychol.*, 1924, 7, 98-116.

Waters, R. H. The law of acquaintance. *J. exp. Psychol.*, 1939, 24, 180-191.

Welch, G. B., and Burnett, C. T. Is primacy a factor in association-formation? *Amer. J. Psychol.*, 1924, 35, 396-401.

Wilson, J. T. Remote associations as a function of the length of interval between learning and recall. *J. exp. Psychol.*, 1943, 33, 40-49.

Wilson, J. T. The formation and retention of remote associations in rote learning. *J. exp. Psychol.*, 1949, 39, 830-838.

Wohlgemuth, A. On memory and the direction of associations. *Brit. J. Psychol.*, 1913, 5, 447-465.

Woodworth, R. S., and Poffenberger, A. T. Textbook of experimental psychology. Mimeog. ed., 1920, 71-72.

Yoshioka, J. G. Weber's law in the discrimination of maze distances by the white rat. *Univ. Calif. Publ. Psychol.*, 1929, 4, 155-184.

V

THE DISTRIBUTION OF PRACTICE AND REMINISCENCE

INTRODUCTION

THE relative efficacy of distributed over massed practice was first demonstrated by Ebbinghaus in 1885. This demonstration presented to Ebbinghaus something of a contradiction within his own data, for the favorable action of rest intervals could not have been deduced from his findings regarding the shape of the retention curve for similar types of material. This contradiction was made more conspicuous in 1897 when Jost's findings appeared to discredit the earlier theories of fatigue and boredom which had been advanced to account for the superiority of distributed practice.

The early investigations of the advantages which followed the spacing of practice were mostly of an exploratory nature. Simultaneously with these early investigations, prolific theorizing was being undertaken. By 1915, no fewer than six separate theories had been designed to account for the results that had been obtained.[1] These theories were, for the most part, of an exceedingly vague nature, in the sense that crucial experimentation upon them was difficult to perform. Evidence of this vagueness may be found in the fact of the vitality of the theories, several of them having survived several apparent death blows to arrive, relatively unscathed, upon the contemporary scene. Exploratory work on the distribution of practice phenomena persisted until approximately 1930 when several factors combined to cause a fundamental change in the nature of the research which has been accomplished since that time.

Reminiscence, although intimately related to the distribution of

[1] It is interesting to note that, during the thirty-year period from 1885 to 1915, only twenty references, either experimental or theoretical, appear in the literature concerning the distribution phenomenon.

practice, and although it offered to the theoretician a type of retention curve more compatible with the implications of the early distribution results, was not isolated as a separate phenomenon until the work of Ballard in 1913. Ballard's work was followed in the next year by Huguenin's (1914) study, but a lapse of nearly ten years took place before reminiscence was again brought under experimental scrutiny by Brown (1923) and Williams (1926).[2] The early work on reminiscence, like the early work on the distribution of practice, was largely exploratory in nature. This phase of the research may be thought to culminate in a study of the conditions of reminiscence by G. O. McGeoch (1935).

During the 1930's, the character of research on reminiscence and distributed practice underwent a change. Fundamentally, this change was related to a general refinement of experimental techniques which was taking place in other areas of the study of learning. The immediate cause of the change, however, was probably the development of coherent and specific learning theories, which gave purpose and direction to subsequent research. Thus, the work of Doré and Hilgard (1937, 1938), Hovland (1938a, 1938b, 1938c, 1939a, 1939b, 1940a, 1940b, 1949), Melton and Stone (1942), and McClelland (1942, 1943), to mention but a few instances, was solidly oriented with respect to a test of theoretical implications, or with respect to the testing of a difference between deductions made from two opposed theories. Thus the *experimentum crucis* came to take the place of the exploratory experiment as the predominant research tool.

From the time when experimentation shifted from the earlier to this later orientation, the differences between distributed practice and reminiscence tended to become minimized. The developing theories, without exception, viewed reminiscence and distributed practice as being different aspects or expressions of the operations of the same factors, thereby making the two sets of phenomena theoretically continuous. Following the work of Ward (1937), the

[2] Studies by Robinson (1921) and McClatchy (1925) conform closely to the present definitions of reminiscence but were published as studies of the distribution of practice.

two also became continuous in terms of the experimental operations by which they were each defined. Subsequent research has made small distinction between them, except that distribution of practice experiments typically have employed multiple intervals of rest, whereas the reminiscence experiments have used a single rest interval.

The Problem of Definition

As we have noted, contemporary theories attempt to explain both reminiscence and the effects of distributing practice in terms of the operations of the same theoretical variables. Prior to 1937, however, there had existed an operational difference between the two sets of phenomena. Reminiscence was defined in terms of an increase in retention with time, following the cessation of learning to a partial criterion. The efficacy of distributed practice was defined in terms of the differences in proficiency which might occur between an experimental group which received distributed practice and a control group which received massed practice. Distributed practice, then, was studied by comparing performances between two groups, one of which received practice at a slower rate than the other. Reminiscence was studied by comparing, within a single group, the levels of performance achieved on successive tests of retention. Following the cessation of practice, an immediate test of retention was given to be followed by a delayed test or tests of retention. Thus the amount of practice afforded by the initial test of retention remained an uncontrolled variable. This was especially true since the test of retention employed was, in practice, different from the method of learning and may have been more effective in increasing performance than an equal amount of additional time spent in studying the material by the usual method, and since the method of complete presentation used in these studies did not afford the experimenter with a knowledge of the course of original learning which could be extrapolated beyond the introduction of the rest interval. The work of Brown (1923) showing that, in making successive recalls of the names of the states of the United States, more states were recalled on the second attempt than on the first, gave

special emphasis to this lack of control. In 1937, however, Ward studied reminiscence using an experimental design similar to the design used in the study of the distribution of practice. This type of design is contrasted with the earlier type in the following diagram:

Early type of design	Original Learning	1st Retention test	Rest	2nd Retention test
Ward-type of design	Group 1	Original Learning	Rest	Relearning
	Group 2	Original Learning	No Rest	Relearning

It should be immediately apparent that the second design possesses several advantages over the earlier form. Most important, it controls the factor of the amount of practice afforded by the first retention test. At the same time it should be noted that the Ward type of design implies the use of different learning methods and materials from those employed in the earlier investigations, all of which employed the memorization of poetry in children by the method of complete presentation. Although it would be possible to study reminiscence using the memorization of poetry or similar material and using the Ward type of design, it is undeniable that this design lends itself but poorly to the use of such materials, and that, with the exception of a single study (Gray, 1940), no experimentation of this type has been performed. Conversely, the earlier type of design has never been used for the study of reminiscence in rote-learning, and rarely for the study of reminiscence in adults. Thus, although the differences in reminiscence which may be attributable to differences of material and method of learning are not, of logical necessity, differences that must appear between the usage of the two types of design, these differences, nevertheless, do exist in historical fact.

Following closely upon its introduction by Ward in 1937, the new type of design was used or advocated by Hovland (1938a, 1938b, 1939a), Bunch (1938), Johnson (1939), and Gray (1940). General acceptance rapidly followed, so that, at the present time, the new

design is firmly entrenched in learning methodology. Both the distribution of practice and reminiscence are now defined in terms of the differences between an experimental group which receives practice interspersed with one or more rests, and a control group which receives continuous practice. In the distribution experiments it is typical for numbers of rests (as, for example, between every trial) to be introduced, whereas in reminiscence experiments a single rest interval is used. The latter phenomenon has become a special case of the former. Pointing this out, Buxton (1942) replotted data from distribution experiments to demonstrate the existence of reminiscence during the first rest period of many studies of distributed practice.

Concerning the definition of distributed practice and reminiscence effects, at least one additional factor deserves mention. The operations involved in experimenting upon either variable imply the prevention or control of implicit practice during the rest periods.[3] Four types of control have been used in the past. The most obvious of these is one in which the subject engages in some form of more or less unrelated activity during the rest periods. This method of control is open to question insofar as different rest-period activities may have different direct and indirect effects upon performance. Color-naming, for example, has been used as an interval-filling activity in a number of experiments. Results obtained by Irion (1949) and Thune (1950) indicate that color-naming may exert a facilitative influence upon subsequent performance, presumably by maintaining the subject's set to perform the learning activity.[4] If rehearsal is to be controlled by filling the rest interval with some unrelated activity, it is probably necessary that the nature of this

[3] This problem also confronts other types of experimentation, as, for example, in the area of retroactive inhibition.

[4] The first suggestion that color-naming might benefit subsequent performance is contained in Ward's (1937) monograph. In order to control possible rehearsal effects in his study of reminiscence, Ward introduced a control group which engaged in color-naming. This group yielded results which were superior to the results obtained from the "rest" group which engaged in light reading during the rest interval. (120 per cent vs. 104 per cent retention). This difference between the groups was statistically significant, being 4.6 times its probable error. Irion (1948) has advanced a possible explanation for this phenomenon.

activity be rigidly specified and standardized, lest the "control" introduce a more serious error than it prevents.

A second method of control for the rehearsal variable is suggested by an experiment performed by Doré and Hilgard (1938). This method involves keeping the total amount of learning time constant, regardless of differences in the number of trials between experimental and control groups. The assumption underlying the use of this method is that rehearsal cannot be more effective than direct practice on the learning task. Thus, if additional overt practice, given to the control group, for the time taken by the rest interval of the experimental group, does not serve to raise the control group scores to the level of the experimental group scores, it is presumptive that implicit practice could not have been the cause of the superiority of the experimental group. This technique is better adapted to certain motor learning tasks which can afford continuous practice than it is to the rote-learning task.

A third method of control involves the questioning of the subjects concerning rehearsal and comparing the results of those who admit rehearsing with the data from those who do not. This method contains the fundamental weaknesses that it is dependent for its effectiveness upon the honesty of the subjects. Even if the honesty of the subjects may be assumed, however, there remains the possibility that rehearsal may be carried on at an unreportable level.

A fourth type of control involves the use of materials which are not as susceptible to symbolic representation, or the use of subjects who, presumably, are less capable of behaving on a symbolic level. The use of non-verbal skills as learning materials, and the use of animal subjects represents an attempt at this type of control.

Recently, the problem of rehearsal has been attacked directly. Rohrer (1949) compared the post-rest performance of a group which had been asked to rehearse during the rest interval with the equivalent trials of a group wherein rehearsal had presumably been prevented. His results indicated that the rehearsal group was not superior to the no-rehearsal group. In fact, the no-rehearsal group enjoyed a slight, although non-significant, superiority. The effects of rehearsal require further study. At the present time, their pos-

sible influence upon results obtained in distribution of practice and reminiscence experiments cannot be estimated with any degree of assurance.

The Pervasiveness of the Distribution and Reminiscence Phenomena

The beneficial effects of distributing practice have been demonstrated under a wide variety of conditions and in a considerable number of learning situations. In the learning of meaningful and meaningless materials, in school learning situations, human and animal maze learning situations, the learning of code substitution tasks, and in the acquisition of motor skills, distributed practice has generally yielded a faster rate of learning than has massed practice.[5] One possible exception to the generality of this phenomenon may be that massed practice is more effective than distributed practice in certain trial-and-error learning and problem-solving situations. That this benefit is probably confined to the early stages of practice is implied by the data of T. W. Cook (1934) on puzzle solution and on maze learning (1936-1937).[6]

It is evident that the effects of distributing practice depend upon a great many variables, including the amount of distribution itself, and it is, no doubt, possible to contrive situations which will reveal a superiority of massed practice. On the other hand, there can be no doubt that the beneficial effects of spacing practice may be demonstrated in a very great variety of situations. So widespread is this phenomenon that it may be stated as a general empirical conclusion for the practical control of learning.

[5] This is particularly true when performance is plotted against amount of practice. When performance is plotted against time (including rests) since the start of practice, it is apparent that, by introducing sufficiently long rests in the distributed practice conditions, massed practice can be made to yield faster learning.

[6] This type of benefit from massed trials may be predicted from Hull's 1932, 1939) theoretical formulations of the trial-and-error learning process. The phenomenon, according to Hull, arises as a result of the fact that the variability of behavior which makes possible the discovery of the correct response is a function of experimental extinction. Massing of extinction trials tends to increase rate of extinction.

The ubiquity of the reminiscence phenomenon has been much less clearly demonstrated. Early studies demonstrated the occurrence of reminiscence in the learning of meaningful material by children. However, as we have already noted, the lack of control in these early studies renders them of doubtful value. That reminiscence may be obtained in the rote-serial anticipation learning situation has been shown by a number of investigators who used nonsense materials. Reminiscence has not been obtained in the serial learning of words, however, and the reminiscence effect has not been observed in paired associate learning.[7] On the other hand, large amounts of reminiscence have been obtained in a variety of perceptual motor tasks.

It is not surprising that reminiscence is more difficult to demonstrate than is the efficacy of distributed practice, or, in other words, that the aggregate effect of several interpolated rests is easier to demonstrate than is the effect of a single rest. Reminiscence probably occurs only under a relatively narrow range of experimental conditions.

THE CONDITIONS OF DISTRIBUTION EFFECTS

As has been stated, the distribution effect may be defined in terms of performance differences obtained under two types of conditions: a condition wherein the rate of practice is relatively low, that is to say, where the units of practice are spaced relatively far apart in time, and a condition wherein the rate of practice is relatively high, the units of practice occurring close together in time.[8] There is, of

[7] It may be maintained that data obtained by Noble (1950) demonstrate reminiscence in the learning of adjectives. Noble, however, feels that his results did not demonstrate reminiscence.

[8] The question may well be raised concerning the units of practice between which it is justifiable to interpolate rests. In rote-learning, for example, distribution may be inter-serial or intra-serial in character. Thus, repetitions of a list may be separated by rests, or the presentation rate of the material may be slowed. Both types of distribution appear to be efficacious and related to each other. Hovland (1938c) investigated this relationship in the learning of nonsense syllables. At a 2-second rate of presentation, the introduction of a 2-minute rest following each repetition of the list proved to be decidedly beneficial. At a 4-second rate of presentation, the introduction of a 2-minute rest in the same manner produced much less effect upon learning. The benefit of

course, no line which separates distributed from massed practice in any absolute sense. The conditions of spaced practice in one experiment may pace practice at a rate considerably faster than the condition of massed practice employed in another experiment. Thus, the distributing or massing of practice is a relative matter, to be determined within the conditions of each experiment. Because of this fact, it is often not possible to draw generalizations beyond the conditions of particular experiments. Considering this discontinuity of method, it seems remarkable that there is as much agreement between experimental results as, in fact, there is. This agreement is probably due, in part, to the fact that the measurements of distribution effects have usually been of the "significantly more" and "significantly less" variety. Undoubtedly, the contradictions of the data would multiply if the formulation of mathematically precise relationships between the effects of distribution and the conditions of it were to be attempted.

Distribution Effects as a Function of the Lengths of the Rest Intervals

If distribution of practice is found to be superior to massed practice, it is obvious that there exists a strong possibility of some distribution intervals being more advantageous than others. Unfortunately, very few experiments have been designed to study the effects of different intervals of rest per se, since many of the studies which have varied length of rest interval have also simultaneously varied the amount of practice between rests, thereby confounding these variables and rendering an analysis of the effects of either of them impossible. Here we shall consider the effects of variation in length of rest period when amount of practice between rests is held constant. In the next section, we shall consider the effects of

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distribution appears, in this latter case, to be consistent, if insignificant. Gundlach, Rothschild, and Young (1927) demonstrated the relationship between speed of presentation and number of errors in the visual memory span situation. Unless otherwise specified, *distribution of practice*, in this chapter, refers to the inter-serial rather than to the intra-serial type.

variations in amount of practice which is introduced between rest periods of standard length.

Leuba and Hyde (1905) studied the effects of varying the length of rest interval upon the learning to transcribe English prose into German script. Constant lengths of practice were used with rest intervals of 12, 24, 48, and 72 hours. The results favored the 24-hour rest intervals early in learning and the 48-hour rests late in learning. In either case, the 12-hour rest was found to be the least effective interval used.

Pyle (1913), in another early investigation, found a twenty-four-hour interval between trials at a code substitution task better than either massed practice or trials on alternate days. This relationship held true independently of trial lengths of 15, 30, 45, or 60 minutes. Pyle also demonstrated, in the learning of arithmetic by third-grade children, that a group which received one 10-minute practice period each day for 10 days was much superior to a group which received two 10-minute practice periods administered twice a day for 5 days.

Lorge (1930), using three types of activity, compared the effect of 20 massed trials with the effect of 20 trials separated by one minute and 20 trials separated by 24-hour intervals. The three tasks were mirror-drawing, mirror-reading, and code substitution. Both the one minute and the 24-hour distribution schedules proved to be superior to the massed practice condition for all tasks. The differences between the two distribution conditions were relatively unstable as compared with the differences between either of them and the massed practice condition. Lorge's results for mirror-drawing are shown in Figure 17.

Using the pursuit-oscillator, R. C. Travis (1937) investigated the effects of 5 minute, 20 minute, and 48, 72, and 120 hour rests introduced after successive periods of 5 minutes of continuous practice. His results indicated that the 20-minute intervals were optimal, the 5-minute intervals yielding the next most advantageous results.

Warden (1923) introduced either a 6 hour, 12 hour, or a 1, 3, or 5 day rest interval after each 1, 3, or 5 trials of maze learning in the white rat. His results indicated that the 12-hour intervals were optimal (or, at least, that the optimal interval must lie between 6 and 24

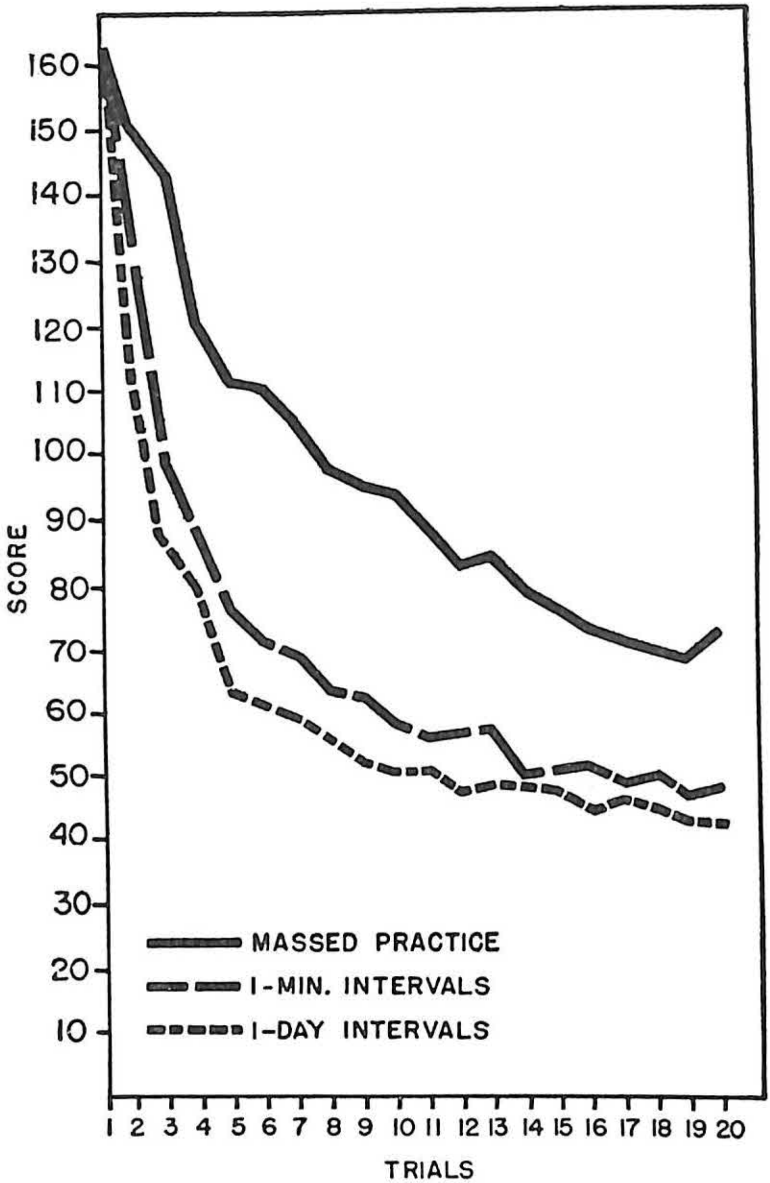

FIG. 17. LEARNING CURVES FOR THREE DIFFERENT DISTRIBUTIONS OF PRACTICE AT A FORM OF MIRROR-DRAWING

(From Lorge, *Teach. Coll. Contr. Educ.*, 1930, No. 438, p. 16)

hours). Using a stylus maze of the same pattern as that used by Warden, Hardy (1930) performed an analogous experiment using human subjects. Her results were not in agreement with those obtained by Warden since she found that the longer and shorter rest intervals were superior to those of moderate length. Hardy's results are atypical of those which have been obtained in similar learning situations and, if verified, they would prove difficult to explain in terms of existing theories of the distribution phenomenon.

Kimble (1949c) has obtained results which indicate that rate of learning is a function of the length of the interpolated rest periods. In learning to write the alphabet upside down, subjects were given either 0, 5, 10, 15, or 30 minutes of rest between 30-second trials of practice. Kimble then obtained the best-fitting straight line for trials 12 to 20 in each group. Taking the slope of these lines as a measure of rate of learning, Kimble found that these rates increase with increasing lengths of rest in an approximately linear fashion.

Wright and Taylor (1949), on the other hand, have obtained different results in the Koerth pursuit rotor situation. They find a marked difference between continuous practice and several distributed practice conditions, each of which employed a different length of rest period. Consistent differences among the distribution groups were not found. This may result from the fact that their range of distribution intervals was not large. It may also result from fundamental differences in the learning situations employed by Kimble, on the one hand, and Wright and Taylor, on the other. Certainly the linear relationship stated by Kimble between length of rest and rate of learning cannot be stated as a general conclusion on the basis of existing data.[8]

[8] In this connection it should be noted that Kimble's task controls inter-trial but not intra-trial distribution of practice. In a sense, this is true of all learning tasks, but it is especially true of self-pacing tasks such as the prod-pursuit task and the one used by Kimble. During each trial of Kimble's experiment, subjects were instructed to accomplish as much as possible during the allotted time. Thus, the subjects who perform best do more work per practice period than those who are less proficient. Whether this tends to minimize or to maximize differences due to distribution of practice is uncertain since no evidence exists and since convincing theoretical arguments may be advanced to support either outcome.

Distribution Effects as a Function of the Amount of Practice Between Rests

Working with nonsense syllables, Jost (1897) gave practice for either 2, 4, or 8 trials a day until a total of 24 trials had been given. His results indicate that the shorter practice sessions are superior to the longer ones as measured by a delayed recall 24 hours after the completion of learning.

Ulrich (1915) distributed practice by the 1, 3, and 5 trial a day method in the learning of a circular maze and a problem box by rats. In the learning of either problem, 1 trial per day proved superior to either 3 or 5 trials per day. In the case of the problem box an additional condition was used wherein 1 trial was given every three days. This condition was found to yield the fastest learning and to be superior to 1 trial per day.

Warden (1923) found the most economical frequency of rest to be a function of the lengths of rest in animal maze learning. For the shorter rest intervals, efficiency was greatest when little inter-rest practice was given. For the longer rests, however, there was a tendency for efficiency to be greater when the practice periods were longer. In a study analogous to Warden's, but using human subjects, Hardy (1930) found that, regardless of the length of the rest interval, small amounts of practice between rests generally yield better results than do larger amounts.

Mayer and Stone (1931) gave groups of young rats and groups of adult rats either 1, 3, 5, or 10 trials per day on a 12-choice multiple-T maze. All animals received a total of 30 trials. The adult rats receiving the 3 and 5 trials per day took more trials to learn than did the adults receiving 1 trial per day. In terms of running times, in both adult and young rats, the 1 trial per day groups were superior to the other groups, especially during the early trials of learning.

Using a single choice-point maze, Lashley (1918) investigated the distribution of practice in rats. One group of animals received 10 trials per day and required, on the average, 57.8 trials to learn,

while a second group which received 2 trials per day required only 21.5 trials to learn. Pre-training, in the form of 20 minutes of exploratory activity on the day preceding the beginning of practice, had the effect of increasing scores under both conditions, but the massed group benefited more from this than did the distributed group. On the other hand, the introduction of a covered dish of food in the cul-de-sac during learning had a slight effect upon the distributed group, but more than doubled the number of trials to learn for the massed practice group.[9]

Kimble (1949b) and Kimble and Bilodeau (1949) have investigated the effects of various trial lengths upon learning. Their subject's task was to write the alphabet upside down. Both studies reveal that shorter trials result in more efficient learning than longer trials. These studies were also designed to determine whether length of practice or length of rest is the more important variable. Conflicting results were obtained in answer to this question, however. Kimble and Bilodeau found that work was the more important of the two variables while Kimble found that rest contributed a greater share. Possibly no generalization can be drawn regarding this problem because of the probability that this relationship depends upon many other factors including the amount of work involved in the performance of the learning task.

In these experiments an important generalization was drawn, namely, that the effect of a particular combination of work and rest is a simple summation of the effects of these variables taken separately. Whether future experimentation will support this conclusion remains to be seen, but certainly it deserves thorough empirical investigation.

[9] This finding strongly supports an interference and differential forgetting interpretation of distribution effects. The covered food dish undoubtedly serves as a secondary reinforcing stimulus, thus causing habit strength to be built up to the incorrect as well as to the correct alley. During rest periods this competing habit should, under the differential forgetting hypothesis, be forgotten at a faster rate than the correct response since it is the weaker of the two. Differential strength may be assumed in this case since it is reasonable to suppose that the secondary reinforcement results in less learning than does the primary reinforcement from which it is derived.

Distribution Effects as a Function of Various Patternings of Practice and Rest

Using a code substitution task, Gentry (1940) compared results obtained under continuous and distributed practice and various combinations of the two. The conditions of Gentry's experiment are outlined below:

Condition	*Procedure*
1.	20 one-minute trials with a one-minute rest after each trial (distributed control)
2.	5 distributed trials followed by 15 massed trials.
3.	5 massed trials followed by 5 distributed trials followed by 10 massed trials.
4.	10 massed trials followed by 5 distributed trials followed by 5 massed trials.
5.	20 massed trials (one-minute practice periods with no intervening rest. This was the massed practice control).

Gentry's results indicated that condition 1 was superior throughout learning. Conditions 2, 3, and 4 tended to coincide with the massed practice curve (produced under condition 5) during the periods when practice was massed, and tended to enjoy a temporary advantage during the periods when practice was distributed. The performance in conditions 2, 3, and 4 approached the curve for condition 1 when practice was distributed. The earlier practice became distributed, the more nearly did performance temporarily approach the performance under condition 1. Following the periods of distribution in conditions 2, 3, and 4, performance fell almost immediately back to the level of condition 5.

Carr (1919) investigated early vs. late massing, using the pencil maze. One group of subjects received 10 trials on the first day of practice followed by 1 trial per day for the next 10 days. The other group received 1 trial per day for the first 10 days of the experiment and then received 10 trials on the eleventh day of practice. The group which received the initial distribution was superior to the group which received the initial massing of practice during the first half of learning, but no differences between the groups were observed on the last ten trials.

Similar results were obtained by Doré and Hilgard (1938), using the Koerth pursuit rotor. One group of subjects in this experiment received practice in which the lengths of the inter-trial rests progressively increased, whereas the other group of subjects received practice in which the lengths of the inter-trial rests progressively decreased. When performance was plotted against trials, the group which received the early massing and late distribution was inferior during the first part of learning and superior during the second part of learning. Thus, whether spacing came early or late in practice, the groups which had the greater distribution showed a temporary advantage.

The results of Renshaw and Schwarzbek (1938) on the Renshaw and Weiss pursuitmeter, however, are in apparent contradiction to the results obtained by Carr and by Doré and Hilgard. Using a design in which the inter-trial rests increased in one group and decreased in another, results were obtained which seemed to indicate that early distribution was more effective than late distribution.

Cummins (1919) studied the learning of various school subjects among children and adults under two patterns of distribution. In one group, which followed the "equal schedule," practice was evenly distributed over a period of time. In the group that followed the "reducing schedule" the same amount of practice was distributed in the same amount of total time, but with the difference that, early in learning, the practice sessions were longer and close together, while, late in learning, practice sessions were shorter and more widely spaced. Under such an arrangement, Cummins found small but consistent superiority of the reducing schedule group over the equal schedule group. Since, however, the variables of amount of work per practice session and length of rest were confounded in this experiment, the obtained results are difficult to interpret.

Distributed Practice as a Function of the Method of Learning

Although distribution of practice effects have been studied under a wide variety of experimental materials and methods, the influence of these factors has seldom been systematically explored. Hovland

(1939b) compared massed and distributed practice in the learning of nonsense syllables by the rote-serial anticipation method and by the method of paired associates. The tasks were equated in difficulty. Serial learning occurred under a 2-second rate of presentation, while paired associate learning took place under a 4-second rate per pair of syllables. Under massed practice, for both methods, 6 seconds intervened between successive trials, whereas under distributed practice, 2 minutes elapsed between trials. Distributed practice was efficacious in rote-serial learning, but no difference between massed and distributed practice could be determined in the learning by the paired associates method. These results, however, may be a function of the fact that the *rate of responding* is different under the two methods. In serial learning, one response occurs every 2 seconds while in the paired associates method of learning, 4 seconds are allowed for each response. Hovland (1949) has recently demonstrated that this factor of response rate is important. Employing the paired associate method, distributed practice was found to be greatly superior to massed practice when the syllables were presented at a rate of 2 seconds per pair. When the rate of presentation was 4 seconds per pair, distribution of practice had a considerably smaller effect.

Distribution of Practice as a Function of the Length and Difficulty of the Learning Task

Lyon (1914) studied the learning of nonsense syllables under onditions of massed and distributed practice. The nonsense sylla-les were arranged in different lengths of list. The results indicated constantly increasing advantage of distributed over massed prac-ce as length of list was increased. The massed practice group, in is experiment, practiced continuously, whereas the distributed ractice group received one practice session per day. Lyon con-uded that the relationship between time to learn and length of list, the case of distributed practice, was approximately linear. Time ent in learning by massed practice, however, varied approximately the square of the length of the list.

Hovland (1940b) studied the effect of distribution of practice in the learning of nonsense syllables which were arranged into lists of 9, 12, or 15 syllables. Under massed practice, a 6-second interval elapsed between successive trials whereas in the distributed practice condition a 2-minute interval was interposed between trials. Distributed practice was demonstrated to be superior to massed practice under all lengths of list, the improvement due to distribution increasing with the length of list (Table IX). Typical serial position curves were obtained under massed practice for all lengths of list. Distributed practice was superior at all serial positions (see Figure 12), but showed most advantage in the central serial positions, consequently the serial position curves were flatter for distributed than for massed practice. This effect was proportionally greater with the longer than with the shorter lists.

TABLE IX

TRIALS REQUIRED TO LEARN THREE LENGTHS OF LISTS BY MASSED AND DISTRIBUTED PRACTICE

(After Hovland, *J. exp. Psychol.*, 1940, 27, p. 272)

	Number of Syllables in List					
	8		11		14	
Method of Practice	M	S.E.	M	S.E.	M	S.E.
Massed	7.00	0.28	11.64	0.68	17.14	0.80
Distributed	5.93	0.36	9.43	0.59	12.14	0.76
Differences	1.07	0.33	2.21	0.62	5.00	0.97

T. W. Cook (1936-1937) studied massing and distributing practice using a Warden U-type finger maze. Maze length was systematically varied. He found advantages for both massed and distributed practice, depending upon the types of measurements employed and the lengths of the maze patterns in question. Massed practice proved to be superior to distributed practice in terms of total errors, on the shorter mazes, and on the earlier trials in learning the longer mazes. Distributed practice was found to be superior when total time scores were compared, especially with the longer mazes and on the later trials.

Retention Following Massed and Distributed Practice

A number of studies have been concerned with a comparison of the degrees of retention of materials learned under massed and distributed practice. In general, material learned by distributed practice tends to be retained better than material learned by massed practice, although for short retention intervals and for certain types of learning tasks, exceptions must be made to this general conclusion.

Cain and Willey (1939) studied the course of retention following massed and distributed learning of nonsense syllables. One group of subjects learned the list of nonsense syllables by practice which was distributed over three days. A second group learned the same list during a single session. Each of these groups was then divided into three sub-groups, the sub-groups being tested for retention after 1 day, 2 days, or 7 days. Large differences in retention at all intervals were found, the distributed group being consistently superior to the massed group (Figure 18).

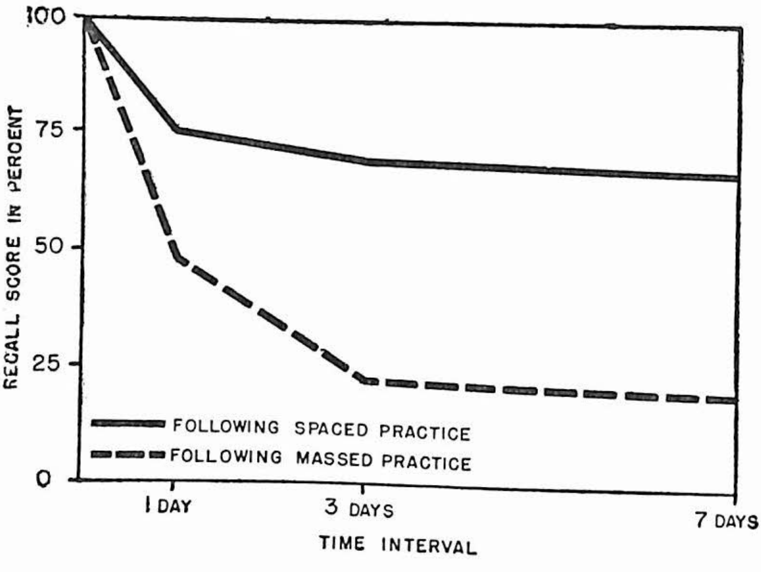

FIG. 18. THE CURVES OF RETENTION OBTAINED FROM THE MEAN RECALL SCORES AT INTERVALS OF 1, 3, AND 7 DAYS FOLLOWING THE MEMORIZATION OF 12 NONSENSE SYLLABLES BY DISTRIBUTED AND MASSED PRACTICE
(From Cain and Willey, *J. exp. Psychol.*, 1939, 25, p. 211)

In 1925 Gordon studied the retention of meaningful material following massed and distributed practice. Her results showed a slight advantage for massed practice groups in tests of retention immediately following learning, but delayed recall tests three and four weeks from the end of practice showed a decided advantage for the distributed practice groups.[10]

Patten (1938) studied the retention of 16 unit lists of nonsense syllables following massed and distributed practice. In the distributed practice group, a 2-minute rest was introduced following each trial. Ten minutes after the end of practice the lists were relearned. In the original learning, Patten found that the distributed practice group learned in significantly fewer trials than did the massed practice group. The serial position curve for the distributed practice group was flatter than that for the massed practice group, thus indicating that distribution was of more benefit to the items in the middle of the list than to the items at the ends of the list. Following the 10-minute rest interval, the distributed practice group recalled significantly more syllables on the first relearning trial than did the massed practice group. There was also some tendency for more anticipatory errors to occur in recall following massed than following distributed practice.

Hovland (1940a) also investigated retention following learning to the same criterion by massed and by distributed practice. Four groups of subjects were required to learn lists of 12 nonsense syllables by massed practice (6 seconds between trials). These four groups were tested for retention at intervals of 6 seconds, 2 minutes, 10 minutes, and 24 hours, respectively, following the cessation of learning. Four other groups were required to learn the lists by distributed practice in which 2 minutes was allowed to elapse between trials. These groups received tests of retention at the same intervals as did the massed practice groups. The retention curve for recall following massed practice showed an initial small decrement

[10] The superiority in retention of the massed practice groups immediately after original learning is consistent with the finding that more reminiscence follows massed than distributed learning. In verbal learning situations, however, this reminiscence effect is very transitory.

followed by a slight rise in retention and then a falling off. The retention curve for the distributed practice groups remained level for a short time and then dropped off with increasing rapidity. At all of the intervals employed, greater retention was obtained following distributed than following massed practice, thus confirming the results of Patten. Hovland also plotted the mean number of errors against serial position. On the first relearning trial following the 10-minute retention interval, the greatest number of errors occurred in the central serial positions. On the first relearning trial following the 24-hour retention interval, more errors occurred at all serial positions than following the 10-minute retention interval, but there did not appear to be any difference between the serial positions with respect to number of errors. On the second relearning trial following the 24-hour interval, however, the greatest number of errors again occurred in the central serial positions.

Robinson (1921) found retention of numbers following massed and distributed practice to be dependent upon the measures of proficiency employed. In general there appeared to be a slight advantage in retention following distributed practice, the advantage increasing with the length of time from the completion of learning.

T. W. Cook (1934) found retention of puzzle solutions to be considerably higher following distributed than following massed practice. The differences tended to decrease rapidly during relearning, however.

THE CONDITIONS OF REMINISCENCE

Reminiscence is defined as an increment in the performance of a partially learned act which is attributable to rest. There are, however, a number of phenomena of the reminiscence effect and a variety of conditions which influence its occurrence. Typical curves of the reminiscence which occurs in rote-learning situations are shown in Figures 19 and 20. This reminiscence effect is a rather transitory phenomenon in the case of rote learning, the rise of retention above 100 per cent and its subsequent fall below that level occurring within a few minutes. There is evidence to indicate that in other learning situations the reminiscence effect persists for

a considerably greater period. Another characteristic phenomenon noted in reminiscence experiments is the rapid increase in proficiency during the first trials following the rest period. Following rest, performance may not only recommence at a higher level than if the rest had not been introduced, but the performance typically increases swiftly during the first period of relearning. This increment was first noted by Thorndike (1914) and was identified by him as a warming-up effect. It has been studied more recently by Bell (1942), Ammons (1947a and b), and Irion (1948, 1949a).

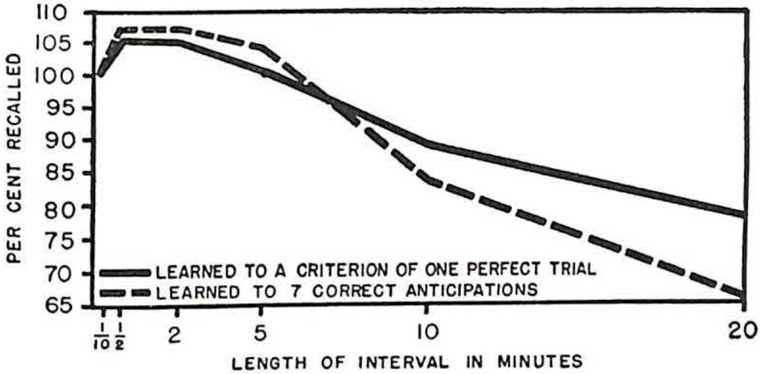

FIG. 19. CURVES SHOWING PER CENT RECALLED AFTER SHORT INTERVALS
(From Ward, *Psychol. Monogr.*, 1937, 49, p. 30)

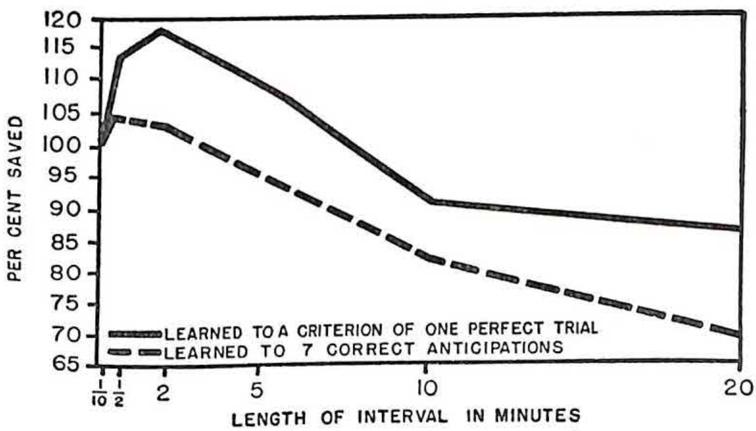

FIG. 20. CURVES SHOWING PER CENT SAVED AFTER SHORT INTERVALS
(From Ward, *Psychol. Monogr.*, 1937, 49, p. 30)

We have seen that, according to experimental procedure, no distinction exists at the present time between reminiscence and the distribution of practice effects. On the other hand, a possible distinction between these phenomena does exist in terms of the *measure* of performance employed. Buxton (1943b) makes such a distinction, referring to increases in recall scores following a single rest as reminiscence, and referring to decreases in number of trials to reach a criterion following a single rest as an effect of distributed practice. This distinction, although legitimate for definitional purposes, appears to be unnecessary and confusing for a number of reasons. Most importantly, such a distinction implies that no improvement occurs within the first relearning trial (or ignores that improvement and the continuity of it with such improvement as may occur between trials). This type of difficulty is particularly evident in learning situations which involve continuous practice on a relatively unitary task such as the Koerth pursuit rotor. In such tasks, it is clear that relearning begins upon the first instant of the resumption of practice after rest and proceeds in a relatively continuous fashion from that time onward until the final cessation of practice, regardless of the number or type of units into which relearning practice is divided. Thus, performance upon the first unit of post-rest practice involves relearning as well as recall. The amount of "relearning" which is included in the "recall" score is a function, in this case, of the length of practice unit chosen by the experimenter. This becomes especially significant in connection with the warming-up effect which has just been noted. The longer the unit of practice chosen by the experimenter (up to a limit), the greater will be the amount of warming-up effect which will be included in the first post-rest trial and the relatively greater loading will this have with reference to a measurement of amount of "reminiscence." [11] It appears useless, therefore, to attempt to separate reminiscence from distribution effects in this manner. An attempt to delimit the reminiscence phenomenon to certain portions of the relearning performance may

[11] Even when the task is one which may be broken up into relatively discrete trials, the same effect may be obtained. In the case of rote learning, this might be done by increasing the length of list which the subject is required to learn.

introduce errors of interpretation which are an artifact of the particular experimental situation employed.

Reminiscence as a Function of Chronological Age

The early studies of reminiscence by Ballard, Huguenin, and Williams were concerned with the retention of children. Ballard (1913) investigated the retention for poetry in subjects who were aged 6, 12, and 21 years. His results definitely indicated a superiority in retention for the younger groups over the older groups. Retention was best in the case of 6-year-old children, the curve showing a considerable increase, rising and falling over a 6-day period, and reaching a high point of nearly 160 per cent on the second, third, and fourth days following learning. Retention in the case of the 12-year-olds showed a slight increase in retention, followed by a slight decrease. Retention for 21-year-olds showed a definite falling off of retention during the 7-day retention interval.

Williams (1926) also studied reminiscence as a function of age. He used four groups of subjects; an adult group, a group with a mean age of 16.2 years, a group with a mean age of 12.7 years, and a group with a mean age of 9.6 years. His results confirmed the earlier results of Ballard, that the younger age groups showed a higher degree of reminiscence than did the older age groups. The retention curve obtained for the group averaging 12.7 years is highly similar to the retention curve obtained by Ballard for 12-year-olds. The retention curve for Williams' 9.6 year group falls between Ballard's curves for 12-year-olds and 6-year-olds. Williams found no differences in his groups when the subjects learned abstract words instead of poetry, nor did reminiscence occur in any of his groups for this type of learning.

G. O. McGeoch (1935) failed to demonstrate any relationship between age (either mental or chronological) and degree of reminiscence. It is possible, however, that the range of ages employed in her experiment was not sufficiently great to demonstrate the existence of the relationship. Also, on the negative side, Magdsick (1936) found no differences in the retention of partially

learned maze problems as between groups of young and adult rats. Reminiscence occurred for intervals of from one hour to one week. Beyond the one-week interval, obliviscence occurred in all groups.

Reminiscence as a Function of the Material To Be Learned

Reminiscence has been demonstrated to occur over a considerable range of learning tasks. Few studies have been concerned with a comparison of amounts of reminiscence obtained for different types of learning materials, however.

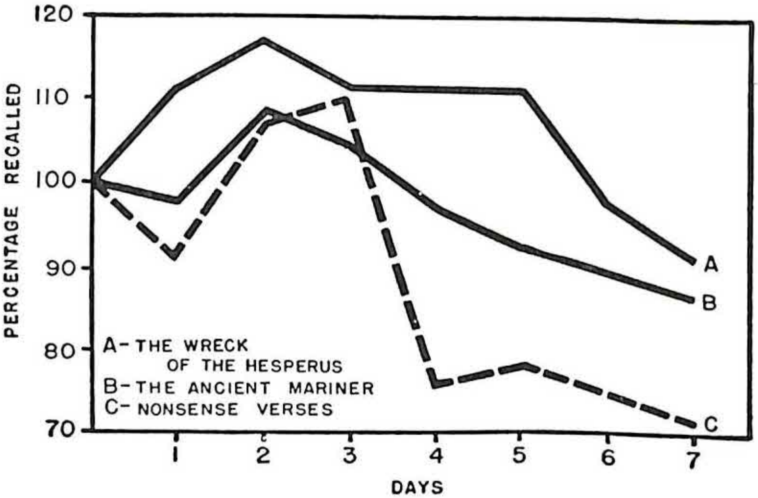

FIG. 21. RETENTION CURVES FOR POETRY SHOWING REMINISCENCE (From Ballard, *Brit. J. Psychol., Monogr. Suppl.,* 1913, 1, No. 2, p. 5)

Williams (1926) found differences between the learning of poetry and the learning of abstract words. The same groups learned both types of materials. In the case of the retention of poetry, the younger age groups showed considerable amounts of reminiscence. For the abstract words, however, no reminiscence was obtained for any age group, nor did there appear to be differences in the shapes of the retention curves obtained for the various groups.

Ballard (1913) found differences in the shapes of the retention curves for various types of material. These differences are illustrated in Figure 21. It will be seen that more reminiscence was obtained for meaningful verse than for nonsense verse.

Reminiscence as a Function of the Method of Learning

Hovland (1939a) compared the amount of reminiscence obtained following rote-serial and paired associated learning of nonsense syllables. Reminiscence was obtained following rote-serial but not following paired associate learning. These results are interpreted by Hovland as favoring a theory of differential forgetting since more remote associations may be formed under the rote-serial-anticipation method of learning than under the paired associate learning. The dropping out of such remote associations during the rest period, Hovland holds, accounts for reminiscence. It should be pointed out that, in this case, the rote serial anticipation method employed a 2-second rate of presentation while the paired associate method used a 4-second rate, 2 seconds for the stimulus member and 2 seconds for the response member of each pair. Moreover, Hovland (1938b) has shown reminiscence to vary with the speed of syllable presentation. He has also demonstrated that, while distribution of practice is ineffective in the paired associate method with a 4-second rate (1939b), it is effective at a 2-second rate (1949). It is, therefore, not unlikely that failure to obtain reminiscence by the paired associate method is a function of the presentation rate used and not a characteristic of the method in other ways. If this is true, Hovland's conclusion regarding the theory of differential forgetting must be regarded as undemonstrated.

Other methodological problems have been investigated by Buxton and his collaborators. These investigations were undertaken, evidently, in an effort to explain some of the inconsistencies which appear between the results of different experiments. Ward (1937), for example, required his subjects to spell the nonsense syllables during learning, while Hovland (1938a, 1938b, 1939a) required that the syllables be pronounced. The effect of this difference in technique was investigated by Buxton and Ross (1949). No significant difference in the amount of reminiscence obtained under these two methods was found. Another difference in technique lies in the fact that Hovland (1938a, 1938b, 1939a) required his subjects to correct all errors committed during the learning process. This correction

had to be made before the subject went on to the next item in the list. Ward (1937) and Melton and Stone (1942), on the other hand, did not use such a correction procedure. Buxton and Bakan (1949) have obtained results which indicate that the correction procedure may produce greater amounts of reminiscence than the non-correction procedure. However, Buxton and Bakan's results are inconsistent with those obtained by Ward (1937), and it seems that a repetition and extension of this work will be necessary before a definite conclusion can be drawn. A third methodological problem involves the nature of the rest period activity. Hovland (1938a, 1938b, 1939a) obtained reminiscence using color-naming during the rest period. These colors were presented to the subject on the memory drum at a two-second rate of presentation. Melton and Stone (1942), on the other hand, did not obtain reminiscence when their subjects read colors from cards as rapidly as possible. This difference in result does not demonstrate the differential effect of these rest period activities, however, since other differences in technique also existed between the two experiments. In a comparison of these two methods of interval filling, Withey, Buxton, and Elkin (1949) found no differences in reminiscence to occur as a function of this difference in rest period activity. They did note, however, that subjects who used Hovland's method of naming colors on the memory drum and who were also instructed to rehearse during this color naming had a tendency to make higher scores than those who were instructed to avoid rehearsal.

Reminiscence as a Function of the Length of the Rest Interval

The work of Ballard (1913) and Williams (1926) indicated that, under certain conditions, the retention curve might rise for a time after the cessation of practice. Their results also suggested, as common sense would dictate, that after a short period of rise, the retention curves would fall. How long after the end of practice the performance increases is a question which these experiments were poorly designed to answer. Using an improved design, Ward (1937)

found the reminiscence effect to be extremely transitory in the case of nonsense syllable learning, occurring for a period of only a few minutes following practice. His curves have been shown in Figures 19 and 20. A comparison of these curves with those obtained by Ballard (Figure 21) reveals the much longer effect obtained by Ballard. Whether this difference is entirely due to the methods employed by Ballard, or whether it may also be a function of differences in the subjects and the learning materials, is not known.

In motor learning situations, the reminiscence effect appears to be more permanent. Bell (1942) varied the length of a single rest introduced after either 5 or 15 1-minute trials on the Koerth pursuit rotor. The trials in this experiment were separated by 1-minute rests. Five lengths of reminiscence interval, 10 minutes, and 1, 6, 24, and 30 hours, were used. After five trials of original learning, varying lengths of rest caused approximately the same degree of initial increase in score. The longer rests, however, caused effects which persisted for a longer time during relearning. Following 15 trials of original learning, short rests appeared to be beneficial, but longer rests caused an initial decrement followed by a quick recovery during relearning. The amount of this warming-up effect increased as the lengths of the rest intervals increased, but showed some tendency to decrease with the 24- and 30-hour rests. This finding regarding the warming-up effect has been corroborated by Irion (1949a) who found that the initial rise in the relearning curve was steeper in the case of long rests than in the case of short ones. Following 20 massed trials on the Koerth pursuit rotor (25 second trials separated by 5-second rest periods), Irion's subjects were given 30 seconds, or 1, 3, or 5 minutes of rest. The increase in amount of reminiscence with increasing rest is shown in Figure 22. Irion's results are strikingly parallel to those obtained by Kimble and Horenstein (1948) under very similar conditions. Ammons (1947) has also obtained highly similar results.

Concerning the action of longer rest intervals, the study of Melton (1941) should be noted. He introduced a single rest of either 20 minutes, 2 days, or 2 weeks early in massed practice on the Koerth pursuit rotor. Under these conditions, the 20-minute rest

yielded the greatest initial amount of reminiscence, but the 2-day rest yielded the best final performance. These results are in agreement with the findings of Bell and Irion regarding the relationship between length of rest and the slope of the relearning curve, and are in general agreement with the findings of Bell relative to the optimal length of a rest interval introduced early into practice.

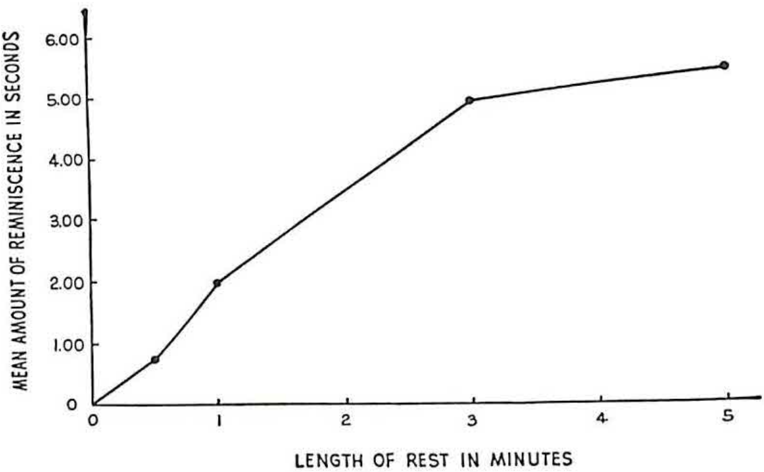

FIG. 22. AMOUNT OF REMINISCENCE IN PURSUIT ROTOR LEARNING AS A FUNCTION OF LENGTH OF REST

(From Irion, *J. exp. Psychol.*, 1949, 39, p. 497)

The values on the ordinate are the mean numbers of seconds on target by which the experimental group exceeded the control group during the first five relearning trials. Each relearning trial was 25 seconds long.

Magdsick (1936), investigating retention of an incompletely learned maze problem in albino rats, found that reminiscence occurred for intervals of from 1 hour to 1 week, following which forgetting took place.

Reminiscence as a Function of the Degree of Original Learning

Two methods have been used to study this problem. In one of these, the experimenter manipulates the degree of original learning directly by having his subjects learn to a certain criterion of pro-

ficiency before introducing the rest period. In the other, the experimenter allows the subjects to practice for a specified number of trials before introducing the rest. There are advantages and disadvantages to each method. The first method serves to reduce the variability of performance at the time the rest interval is introduced and, in general, exerts strict control over the level of proficiency attained by the subjects at this time. On the other hand, total amount of pre-rest practice is not controlled, and this may serve to introduce variability in terms of different degrees of warming up. The second method, while it gives an equal amount of practice to all subjects, does not exert a very powerful control over level of proficiency.

Ward (1937) investigated the amount of reminiscence which occurred following the partial learning of lists of nonsense syllables. He found amount of reminiscence to be an inverse function of the degree of original learning. It is quite possible, however, that Ward's subjects showed less reminiscence at high levels of proficiency because they were "running into an upper limit, i.e., 100%."

Buxton (1943a), in studying reminiscence on the Koerth pursuit rotor, had three groups practice to different criteria of proficiency. Following this, each group received a 10.5-minute rest, after which each group relearned to its own criterion. Scores on the three groups were then compared with scores obtained under conditions of massed practice. Significant amounts of reminiscence were obtained for all groups. The groups with the smallest amount of original proficiency showed more reminiscence than the group with the highest degree of original proficiency.

In similar experiments, Ammons (1947) and Irion (1949a) investigated the effects of amount of pre-rest practice on reminiscence in the Koerth pursuit rotor. Using 25-second trials separated by 5-second rests as the basic massed practice condition, Irion compared scores from a group which received 45 massed practice trials with scores from five groups which received a single 5-minute rest following 2, 10, 20, 30, and 40 trials of practice. The results indicated that amount of reminiscence at first increases and then decreases as amount of pre-rest practice increases. These results are shown in Figure 23. Analysis of Irion's data reveals that the relearning curves

have steeper slopes in the initial segments following large amounts than following small amounts of original practice. In other words, the warming-up effect increases as amount of pre-rest practice increases.

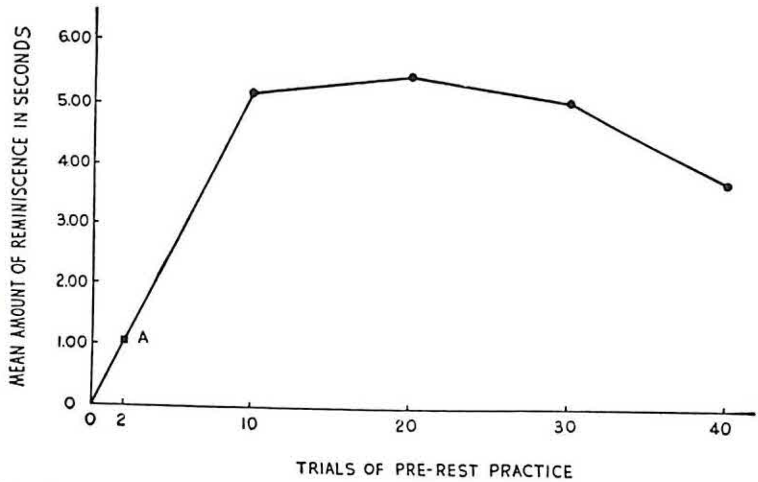

FIG. 23. AMOUNT OF REMINISCENCE AS A FUNCTION OF AMOUNT OF PRE-REST PRACTICE

(From Irion, *J. exp. Psychol.*, 1949, 39, p. 496)

The values on the ordinate are the mean numbers of seconds on target by which the experimental group exceeded the control group during the first five relearning trials. Each relearning trial was 25 seconds long. The point labeled A was obtained in a separate experiment.

Ammons' experiment yielded very similar results, although his type of experimental design, while more complete, yielded less regular functions.

Bell (1942) also studied this relationship. His results showed that amount of reminiscence at recall was a function of the amount of original practice. Following 5 trials of original practice, large amounts of reminiscence were obtained regardless of the length of rest interval used. Following 15 trials of original practice, smaller amounts of reminiscence were obtained for the short rests while the use of longer rests resulted in recall decrement.

McClatchy (1925) interpolated a 48-hour rest following either 1, 3, 5, 7, 9, or 11 trials on a stylus maze. Interpolated rest caused an immediate decrement wherever it was introduced. A delayed

effect of rest might be beneficial or detrimental according to the amount of original learning. The amount of pre-rest practice which appeared to be optimal for the beneficial operation of the 48-hour rest was 7 trials. Decrement followed the introduction of a rest of this length at other stages of practice. In a second experiment, McClatchy used a 24-hour rest instead of a 48-hour rest, all other procedures being identical. The optimal locus of the 24-hour rest appeared to be in the earliest trials. The 24-hour rest did not cause decrement unless it was introduced following either 9 or 11 trials of pre-rest prcatice.

Kimble (1949a) has studied reminiscence as a function of amount of pre-rest practice. His results show a tendency for amount of reminiscence to decrease as amount of pre-rest practice increases. One interesting feature of this experiment is the inclusion of a well-distributed practice condition into the design. This allows a comparison of post-rest performance in the reminiscence conditions with performance during acquisition by distributed practice. Kimble's findings show that, when a rest interval is given early in practice, reminiscence occurs in such amounts as to make post-rest performance approximately equivalent to the spaced practice performance for the equivalent trials. When a rest interval is inserted late in practice, however, the reminiscence which occurs does not yield a post-rest performance which approaches the performance of the distributed practice group. This, Kimble interprets as evidence for the existence of a permanent decremental factor in motor learning.[12]

Reminiscences as a Function of the Previous Degree of Distribution of Practice

As has been noted previously, it is possible to distribute practice in a number of ways. In rote-learning, for example, it is possible to distribute practice by decreasing the presentation rate of the mate-

[12] Such a permanent decremental factor may be identified with Hull's concept of conditioned inhibition ($_{S}I_{R}$), which is an habitual avoidance of a way of responding. This avoidance is based upon relief from fatigue as reinforcement.

rial or by introducing rests between successive presentations of the entire list. Both types of distribution will be considered in this section, insofar as distribution is related to reminiscence.

McClelland (1942) studied reminiscence in a type of verbal discrimination learning situation. He found reminiscence following a 2-second rate of pair presentation, but not following a 4-second rate. McClelland presented his subjects with pairs of words. The subject was required to speak one of these words and received reinforcement if he chose the correct member of the pair, reinforcement being administered immediately after the withdrawal of the stimulus words. Following learning to a criterion of 15 correct choices out of 20 possible choices, a 2-minute rest was introduced into the reminiscence condition. Under these circumstances, rate of presentation of the original material acts as a determiner of reminiscence following the 2-minute rest interval.

Hovland (1938b) studied reminiscence in rote learning as a function of the presentation rate of the material to be learned. One of Hovland's groups learned a list of 12 nonsense syllables to a criterion of 1 perfect trial under a presentation rate of 2 seconds per syllable, while a second group learned to the same criterion under a 4-second presentation rate. The two groups learned (at either a 2-second or 4-second rate) to a criterion of 7 syllables correct before receiving a 2-minute rest. Reminiscence was obtained with the 2-second, but not with the 4-second, rate of presentation. When mean number of failures was plotted against the serial positions of the syllables for the trial following the attainment of the partial criterion in the massed practice control groups, it was found that many more errors occurred in the central portion of the list with the 2-second than with the 4-second rate. Differences in the terminal serial positions were not so marked. In the case of the experimental group on the same trial (which, in this case, was the trial immediately following the 2-minute rest), the serial position curve for the 2-second presentation rate group showed a marked degree of flattening whereas the serial position curve for the 4-second presentation rate groupe showed no such flattening.

Hovland (1938a) also investigated the effect of varying the inter-

trial interval. Using the same general design as was used in the experiment cited above, Hovland's massed practice group received 6-second rests between trials whereas the distributed practice group received 2 minutes and 6 seconds between trials. A 2-second rate of syllable presentation was used throughout the experiment. The results which were obtained are in essential agreement with those obtained under different presentation rates. Reminiscence was found following massed practice but was not obtained following distributed practice. The serial position curve, in the case of massed practice, showed a considerable amount of flattening as a result of the introduction of the rest, but this result was not obtained under the conditions of distributed practice.

Little has been done in the field of motor learning concerning the effects of distribution of practice upon reminiscence. Reyna (1944) has shown, however, that the same general relationship exists in motor learning situations as has been found to be true of the verbal learning situations, the results of which have been cited above.

THEORIES OF REMINISCENCE AND THE DISTRIBUTION EFFECT

A considerable number of theories have been developed to account for the effects which have been discussed in this chapter. These theories possess value in that they unify and simplify the relationships and facts which have been observed and in that they serve as a stimulus to further research. Although any classification of these theories necessarily does violence to some of them while, possibly, overemphasizing the importance of others, it is apparent that the theories do fall into certain groups. We shall classify these hypotheses under three headings: work theories, perseveration theories, and differential forgetting theories.

Work Theories

As a group, work theories hold that performance of an activity tends to leave behind it some process or product in the organism which tends to prevent recurrence of the activity or which tends

to lower the efficiency of its performance. The interfering process is held to dissipate during periods of inactivity while the learned habits are assumed to be more permanent. Thus, the interpolation of a period of inactivity tends to benefit the performance of the learned act. The simplest type of this theory is the fatigue theory.

(A) *The fatigue theory.* One of the first possibilities to occur to the early experimenters was that the fatigue engendered by massed practice was responsible for its inferiority to spaced practice. The latter, through its shorter practice periods and interspersed rest intervals, might produce less fatigue and give time for recovery from whatever fatigue was produced. The concept of fatigue has an ambiguous status in this context. Does it mean the accumulation of fatigue products in the neuromuscular mechanisms which are basic to learning; does it mean a work decrement which is, in turn, a function of inhibition and other conditions; or what does it mean? The first meaning, the accumulation of fatigue products, will be taken here, since the meaning which reduces to inhibition and related conditions is more relevant to a later theory.

Jost (1897) found distribution still superior to massed practice when the subject had done as much work during the total time covered by distribution as he did during unspaced practice. Further, in few, if any, experiments has practice with zero distribution extended over enough time to produce any important amount of physical fatigue. Even when an interval has been introduced after one or two short practice periods as in the work of Hovland (1938a), Ammons (1947), and Irion (1949a), beneficial effects of that rest may be demonstrated. Furthermore, the fact that a single rest introduced late in practice has often been found to have a less beneficial effect than the same rest introduced early in practice is embarrassing to the theory, since, presumably, there is more fatigue from which to recover late in practice than early in practice.

Another line of evidence opposed to a fatigue theory is found in those distribution and reminiscence experiments which require the subject to do the same or extra physical work during the rest periods as during practice. Nevertheless, beneficial effects of "rest" may still be demonstrated. The fatigue theory cannot account for

the results obtained in experiments on the influence of the locus of distribution in the practice continuum, or for those on the influence of the length of the rest interval, especially when the intervals compared are all long enough to have permitted dissipation of physical fatigue.[13]

The fatigue theory cannot be seriously considered as being an explanation of the obtained results. It might be a factor in the ineffectiveness of very long periods of continuous practice, if such periods were ever to be used, but it can hardly be a general determining condition of the efficacy of distributed practice or the occurrence of reminiscence.

(B) *Reactive inhibition theories.* Very similar to the fatigue theory, but usually more precisely formulated, are the reactive inhibition theories. In terms of animal learning theory, Hull (1943) has defined reactive inhibition in terms of the number of responses made by the learner, the amount of work performed during each response, and the length of time which has elapsed since responding.[14] Amount of reactive inhibition present at any moment subtracts from the reaction potential which exists to perform the response in question. Since reactive inhibition increases during performance and dissipates during rest, Hull is enabled, by this means, to explain many of the phenomena of conditioned response learning such as experimental extinction and spontaneous recovery. This form of explanation, of course, may also be applied to the distribution and reminiscence effects. Kimble and Horenstein (1948) have applied Hullian theory directly to the interpretation of pursuit rotor reminiscence data and have obtained a rather good fit to their empirical points by the use of Hull's rational equation for the decay of reactive inhibition.

Ammons (1947a) has elaborated a theoretical system specifically

[13] Renshaw and Schwarzbek (1938) discuss fatigue as a condition of the efficacy of distribution in learning to operate a pursuitmeter and reject the fatigue hypothesis. For an excellent review of the general problem of the effects of work upon behavior, the reader should consult Solomon (1948).

[14] The concept of reactive inhibition had been formulated prior to Hull's work by Miller (with Dollard) in 1941 and by Mowrer (with Jones) in 1943, and is sometimes referred to as the Mowrer-Miller hypothesis.

designed to account for human motor learning phenomena. His concept of temporary work decrement is analogous to Hull's concept of reactive inhibition, although it is not so rigidly defined. This temporary work decrement is held to increase during practice and to dissipate during rest, thus affording a theoretical basis for distribution of practice and reminiscence phenomena. Ammons has also incorporated into this system a set of assumptions regarding the warming-up effect, which, as we have noted, is an important feature of motor performance curves. These assumptions, together with others, yield rather precise predictions of reminiscence effects.[15]

On the other hand, evidence that reactive inhibition or fatigue probably does not offer a complete account of the reminiscence phenomenon, even in motor learning situations, has recently been obtained by Gustafson and Irion (1951). This evidence involves the occurrence of reminiscence in the bilateral transfer of training situation. Two groups of subjects received 10 trials of practice on the Koerth pursuit rotor using the right hand. Both groups then shifted to the left hand and received 10 additional trials of practice. One group made this shift immediately after completing the right-handed trials. The other group, however, was allowed to rest for 5 minutes before shifting to the left hand. The results (Figure 24) indicate a clear superiority of the latter group during practice with the left hand. While it is probably best to interpret these results with caution until further evidence regarding this effect becomes available, it would seem to be clear that reminiscence cannot be entirely explained in terms of the peripheral fatigue of the effector involved. It may be that a generalized reactive inhibition can explain the results, or that this effect may be due to recovery from reactive inhibition located in the perceptual mechanisms involved in such learning.

(C) *Motivation.* The continuous work demanded by massed practice may be accompanied by declining motivation, which, in

[15] The reactive inhibition hypothesis is similar, in many ways, to the differential forgetting theories which are discussed in a later paragraph. In fact, an excellent case could be made for classifying the reactive inhibition hypothesis as a differential forgetting theory.

turn, operates to produce a decreased rate of learning or a poorer level of performance. The rest intervals of positive distribution may, on the other hand, permit the subject to return to the practice situation with a higher motivation and a resultant increase in efficiency (cf. Mowrer, 1940). This is a plausible hypothesis where very

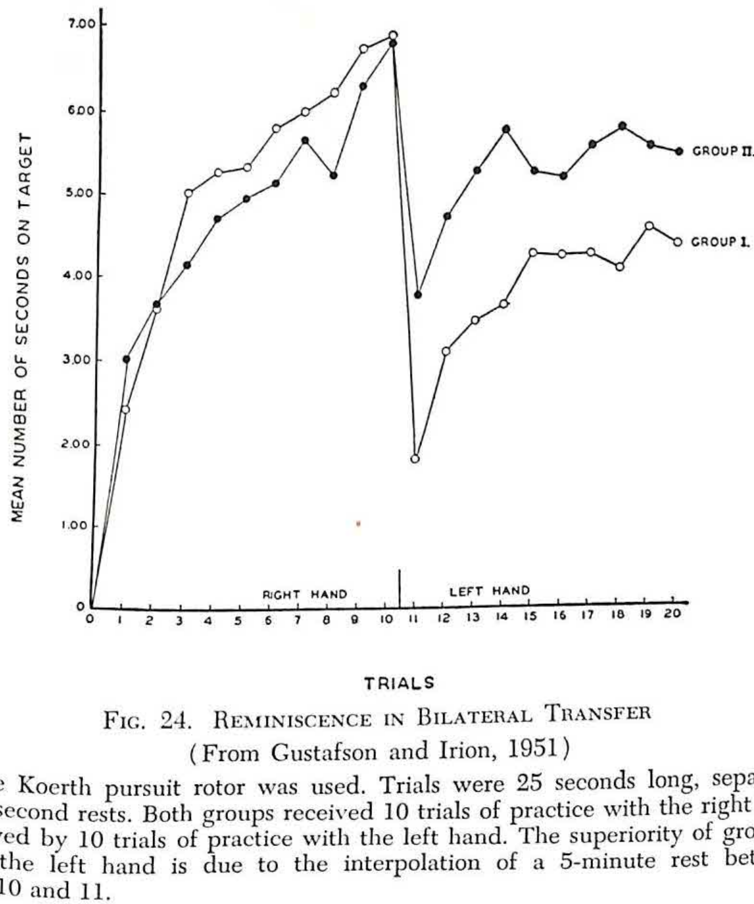

FIG. 24. REMINISCENCE IN BILATERAL TRANSFER
(From Gustafson and Irion, 1951)

The Koerth pursuit rotor was used. Trials were 25 seconds long, separated by 5-second rests. Both groups received 10 trials of practice with the right hand followed by 10 trials of practice with the left hand. The superiority of group II with the left hand is due to the interpolation of a 5-minute rest between trials 10 and 11.

long and arduous massed practice is compared with positive distribution, but it does not apply with any cogency where short periods of massed practice are compared with the still shorter practice periods of positive distribution. It is insufficient to account for the results of Hovland's series of studies on distribution and reminiscence and for many of the other phenomena of distribution. Dif-

ferences in motivation may be present in experiments on this problem and may be a condition of some of the results, but it is doubtful if these differences constitute a general condition which determines the outcome in the majority or in all of the cases.

Differences in quantitative set, as described by Bills and Brown (1929) in the field of work-effects, may also be an occasional condition of results in distribution of practice experiments. They find that the initial level of efficiency and the steepness of the decrement in adding is directly proportional to the subject's quantitative set, *i.e.*, the *amount* of work with which he is faced at the beginning. If the subject knows whether he is to employ spaced or unspaced practice, his quantitative set will be a function of this knowledge, and his rate of learning under massed practice may be less as a result. Since this has never been tested under the conditions of distribution, nothing more need be said about it here, except that many of the objections to motivation as a general condition of distribution effects also weigh against the quantitative set hypothesis. Certainly, such an hypothesis could explain only a small portion of the data and would be quite inadequate to explain, for example, the results of animal experimentation on the distribution of practice.

(D) *Refractory phase.* The discovery of a refractory phase in associative processes (Dodge, 1927; Thorndike, 1927; Telford, 1931), *i.e.*, the existence of a barrier against immediate repetition of a response which has just been made, reveals a condition which is possibly unfavorable to massed practice. One line of reasoning is that, in massed practice, the subject has to repeat the practiced responses soon enough to bring them within their refractory phases and hence brings about resistance to repetition. The few studies which have been made of refractory phase in associative processes have dealt only with isolated responses and do not tell us whether it exists at all in serial learning where time filled by interpolated responses fills the interval between repetitions. Nor do they tell us whether the length of the phase is great enough to have any influence in practice at activities whose repetition consumes more than a few seconds. For example, the effects of distribution in verbal

learning experiments should decrease as the length of list increases, since this allows greater time to elapse between the repetition of each item. Actually, however, distribution effects increase with increasing length of list (Hovland, 1940b).

There is another manner in which refractory phase may have an effect upon learning. This is in terms of the effects of massing of practice upon response variability. The refractory phase hypothesis should lead to the conclusion that massing of trials should lead to increased variability of response.[16] If this conclusion is assumed to be true, there still remains the problem of determining the effects of such increased variability upon learning. Ericksen (1942) has hypothesized, on the basis of such an assumption, that learning problems which benefit most from initial variability of attack should be most efficiently learned under massed practice. This hypothesis he has confirmed with human subjects using the McGeoch problem box. T. W. Cook (1934) has found massed practice to be superior to distributed practice in early trials of puzzle-solving, a type of situation presumably calling for initial variability rather than stereotypy of attack.

At the present time, however, insufficient evidence exists to evaluate the refractory phase hypothesis in a straightforward manner. Refractory phases seem to belong in the context of work decrements, and many of the arguments which tell against the fatigue hypothesis weigh against this hypothesis also. It is, however, a phenomenon which deserves further study.

[16] The problem of variability of response as a function of the massing of trials has received considerable attention in a number of contexts at both the human and the animal levels. Apart from the studies already mentioned, the reader should consult Lashley (1917), Robinson (1934), Hunter (1914), and Heathers (1940). Solomon has recently published an excellent review of this material (1948). It may also be noted in passing that Book (1925) advanced an hypothesis which is the exact opposite of the refractory phase hypothesis. His contention was that concentrated study resulted in more stereotyped rather than in more variable behavior.

Perseveration Theories

Perseveration theories are characterized by the assumption that some form of activity persists after practice, which causes the learned response to become more firmly fixated than it was at the termination of formal practice. There are several varieties of this type of hypothesis.

(A) *The classical perseveration theory.* Müller and Pilzecker (1900) formulated a perseveration theory to account for many phenomena of retention, and the theory has since been invoked as an explanation of distributed practice. In its original form, the theory assumed that the neural activity involved in learning persists for some time after the cessation of formal practice. As a result of this continuing activity, the neural pattern becomes "set" or more firmly fixated. Massed practice does not give time for this "setting in" process to go on to its full extent, while the introduction of a rest interval allows it to become complete. Positive distribution is, then, more effective than zero distribution because it takes advantage of the perseverative "setting in" of the neural traces. This "setting in" process amounts to a kind of unmeasured and unintended practice. Reminiscence, of course, is explained in the same way.[17]

Proponents of this theory do not usually attempt to specify the character of perseveration as a series of neurochemical changes or events. Usually, no statement is made concerning the experimental events, if any, of which perseveration is a function. Reference is often made, instead, to conscious and behavior phenomena which are alleged to point to the reality of some kind of neural perseveration. Persistent after-images are held to be important evidence by virtue of the unique embryological relation of the retina to the cerebrum. Of similar import are the "running of tunes in one's head," strongly recurrent memory images, persistent recall or thinking about acts recently performed or things recently experienced, together with the tendency of many forms of behavior to continue,

[17] For a recent and ingenious variation of the perseveration theory, see Von Förster (1948).

once they are begun. Acceptance of such data as signs of a general neural perseveration in learning outruns the facts, since it is not at present known whether there is perseveration of the neural substrate of learning.

The theory has the merit, however, of being a very general one which, if it were acceptable, would account for many other phenomena besides those of distribution and reminiscence. Furthermore, the theory does explain certain phenomena adequately and, in at least one case, gives more adequate prediction than do other (nonperseverative) theories. The theory can account for Spight's (1928) results on the influence of the filling of the rest interval with sleep or with waking activity, for the fact that intervals of 1 or 2 minutes are often found to be nearly as effective as much longer ones, and for the conclusion that the wider forms of distribution are the more effective ones. Furthermore, results obtained by Duncan (1949) on the effects of electroconvulsive shock upon the retention of habits in the rat strongly support a perseveration theory. For example, in the learning of a simple habit, electroconvulsive shock was given at various intervals following the trials of practice. These intervals were 20 seconds, 40 seconds, 1, 4, and 15 minutes, and 1, 4, and 14 hours. Convulsive shock administered 1, 4, or 14 hours after each trial of practice had no significant effect upon learning, but learning was definitely slower for the other groups. Furthermore, this depression of the rate of learning increased as the time between the practice trials and the shock decreased so that the 20-second group showed the slowest learning, the 40-second group was next slowest, and so on. It would seem that, under Duncan's experimental conditions, rate of learning is a negatively accelerated, increasing function of the time interval between trial and electroconvulsive shock. These results are, of course, in agreement with observations upon human patients suffering from retrograde amnesia as a result of electroconvulsive shock or other neural injury. They offer strong support for a perseveration hypothesis, although it may be possible to formulate another explanation for these results.

Against a perseveration theory, a considerable array of facts may be mustered. The theory offers no explanation for the lack of a

difference between spaced and unspaced practice when recall is relatively early after practice and for the subsequent appearance of a clear difference after a longer time, nor does it account for the increase in the superiority of distribution of practice with increasing amounts of material. The theory is indefinite, moreover, on a number of necessary points. The interpolation of a rest interval after only a single repetition of fairly complex material has been found to be beneficial. In this case, where very little measured learning has occurred, what is there of the pattern to perseverate? This leads at once to the question of the way in which the activity in process of being acquired perseverates. Does it do this as a unit or in parts? In either case, the time relations of the parts raise a puzzling question. Do the first parts of the material start perseverating in the order in which they are gone through during practice? If so, succeeding parts should diminish the "setting-in" of their predecessors, and there should emerge a serial position curve of correct responses which has its lowest point at the beginning of the material and its highest point at the end. Except for the backward elimination of errors found in maze learning with terminal reinforcement, which backward elimination is commonly explained on other grounds, such a serial position curve has not been found.

At the present time, the perseveration hypothesis must be regarded as unpromising except for the results which have been obtained by Duncan. These results offer strong support to the theory, and, at the present time, make it impossible to reject the perseveration hypothesis completely. On the other hand, the theory runs into many contradictory facts which make its acceptance difficult. It is to be hoped that the promising research opportunities which have been opened up by Duncan's experiments will be diligently pursued in order that a resolution of this difficulty may be achieved.

(B) *Rehearsal.* Another form of perseveration theory differs from the one just described in that it identifies the perseverative process as implicit practice or rehearsal. Rehearsal has long been recognized as a possible explanation of the results of reminiscence and distribution of practice experiments. The theory has the virtue of sim-

plicity, the gains still being attributed to practice, in this case uncontrolled and unmeasured, which occurs during the rest interval. Unfortunately, however, the theory cannot explain several different lines of evidence.

One crucial objection to this theory is the fact that distribution has been found to be superior to massed practice in a number of experiments with animals in which one cannot reasonably assume any active rehearsal during the rest intervals. In many activities on the human level, such as the pursuit rotor, implicit practice may be similarly ineffective, since the symbols, by means of which symbolic practice could occur, are largely lacking. It is noteworthy that in the case of the pursuit rotor and other motor activities, distribution is unusually effective and that large amounts of reminiscence are obtained. The results obtained by Spight (1928) may also be noted. It is unlikely that rehearsal should account for the superiority of an interval partially filled with sleep over one filled entirely with waking activity.

Furthermore, attempts to control the rest interval activity in such a manner as to prevent rehearsal (Hovland, 1938a, b, c; 1939; 1940a, b, for example) have not resulted in equality of learning between massed and distributed practice groups.

Lastly, the results obtained by Rohrer (1949) have indicated that the common assumption to the effect that rehearsal benefits learning may be in error, at least under certain circumstances.

In view of these facts, it must be considered that the evidence is definitely opposed to the rehearsal hypothesis.

(C) *The Snoddy hypothesis.* As a specialized form of the perseveration point of view, the theoretical formulations of Snoddy (1935) should be noted. Snoddy attempts to account for the phenomena of distributed practice and reminiscence in terms of the interacting effects of two opposed processes of "mental growth." These two processes, called primary and secondary growth, have different properties. Primary growth occurs early in practice and increases as length of interpolated time increases. Secondary growth occurs later in practice and is maximum when practice is continuous. The concept of primary growth, formulated in this way,

will be seen to be closely related to the concept of perseveration as developed by Müller and Pilzecker, not so much in terms of the assumed physiological mechanisms involved as in terms of formal theoretical properties. This theory has been the subject of a considerable amount of experimentation and discussion. (Bell, 1942; Doré and Hilgard, 1938; Hilgard and Smith, 1942; Humphreys, 1937; Renshaw and Schwarzbek, 1938; and Snoddy, 1938). The theory is able to account for many of the phenomena of distributed practice and reminiscence. It is, furthermore, considerably more specific than the older forms of the perseveration theory. Its primary weakness is one which is common to all theories which postulate (in non-specific terms) two opposed processes; namely, that skillful manipulation of these factors enables the theoretician to predict almost any result. The theory has suffered, furthermore, from the experimental results of Doré and Hilgard (1938) and has been damaged by their theoretical analysis of it. At the present time, Snoddy's theory must be regarded as unpromising, especially because the lack of specific elaboration of it tends to render it fruitless as a basis for theoretical organization and further experimental analysis.

(D) *The stimulus-maturation hypothesis.* Very similar to Snoddy's theory is the principle of stimulus induced maturation which has been proposed by Wheeler (1929) and Wheeler and Perkins (1932). Under this view, the perseverating process is a form of growth or maturation, induced by stimulation, but relatively independent of *rate* of stimulation. A prediction from Wheeler's hypothesis is that rate of improvement in a learned task should be a function of the time elapsed since the beginning of practice rather than of the number of trials occurring within that time. It is obvious that this hypothesis can apply only within rather narrow limits, if at all, since by spacing the trials sufficiently far apart, rate of improvement can be slowed almost to zero. Early results obtained by Doré and Hilgard (1937) and by Hilgard and Smith (1942) tended, nevertheless, to confirm the hypothesis. Later results obtained by Bell (1942) and especially those obtained by Wright and Taylor (1949) do not support the hypothesis. The reason for this

disagreement of results probably lies in the fact that if a rest interval can be more productive of improvement than an equal amount of time spent in formal practice (a result which may be experimentally demonstrated), and if wide spacing of trials results in slower learning, plotted against time, than does continuous practice (an assumption which appears to be eminently reasonable), then Wheeler's prediction is bound to hold approximately true over a limited range of conditions. If these assumptions are granted, then it follows that there must be a degree of distribution of practice at which these two factors are in approximate equilibrium and Wheeler's generalization will hold approximately true for the distribution intervals which surround this point. To argue that positive results, so obtained, support Wheeler's theoretical superstructure or that negative results obtained outside this range of conditions result from "overstimulation" or "understimulation" is futile since the whole range of results may be predicted on purely empirical grounds.

Differential Forgetting Theories

During the course of practice a subject learns not only the correct responses, but also incorrect and conflicting ones which retard the fixation and interfere with the performance. Since these conflicting associations may be expected to be less well fixed than the correct ones, it is assumed that they should be forgotten at a faster rate during rest intervals such as those introduced in reminiscence and distribution experiments. Easley (1937) has demonstrated the reasonable nature of this assumption by showing that poorly learned associations are forgotten at a more rapid rate than well learned ones.[18] It follows, then, that learning should be faster under distributed practice than under massed practice because the

[18] It should be emphasized that the differential forgetting theory involves the assumption that weaker responses are forgotten at a faster *rate* than stronger ones. Slope of the retention curve is held to be a function of original level of associative strength. A second assumption is that conflicting and incorrect responses are, on the average, weaker than correct responses. Thus, conflicting responses are forgotten more rapidly because they are weaker and not because they are incorrect.

rest intervals give opportunity for this differential forgetting to occur. A further implication of the theory is that, if practice is distributed too widely, the benefits of distribution will disappear and further distribution will become disadvantageous. This is because, after a considerable interval of time, the conflicting associations have been reduced to a strength approaching zero where little further forgetting of them can occur. The stronger, correct associations, however, may still be forgotten and the resulting decrement results in a net loss rather than a net gain. Massed practice, it is held, either strengthens conflicting associations or permits them to remain longer at a relatively higher level of strength and therefore requires more practice to bring the correct responses to dominance. Reminiscence, of course, is explained in terms of the operation of differential forgetting during a single rest rather than during multiple rests. This hypothesis may also be stated, as Hull (1935, 1940) and Hovland (1938a) have done, in terms of the building up of "excitatory" and "inhibitory" processes during practice and of the more rapid dissipation of the "inhibitory" processes with time. These "inhibitory" processes, according to this view, have approximately the same properties as has the inhibition of delay. Whichever statement of the differential forgetting hypothesis is accepted, the diminution of conflicting or inhibitory conditions at a faster rate than that of the correct or excitatory conditions will leave the latter at a relatively greater level of strength after the lapse of an interval. Although these two points of view have essentially the same formal properties, the present writers tend to favor the competition of response point of view.[19]

A few of the many different conflicting associations which may be formed during practice should be mentioned. Wrong movements

[19] There is no overwhelmingly good reason for favoring either of these viewpoints over the other one. Certain experimental evidence seems to favor the competition of response point of view (cf. Lashley, 1918, and McGourty, 1940). On the other hand, Hull's position has been elaborated in specific, quantitative detail and is likely, therefore, to be more fruitful in terms of future research. It may also be noted, again, that the reactive inhibition hypothesis is, in a sense, a differential forgetting theory since reactive inhibition is held to be less permanent than the reaction potential from which it subtracts.

and place associations are frequent in perceptual-motor learning. Examples are retracing or making a wrong turn in a maze, incorrect spelling or the striking of a wrong key during typing, and the fixation of a relatively ineffective grip or stance in the operation of a pursuitmeter or at golf. Coordinate with these are ineffective modes of procedure, carelessness, incaution, faulty perceptual habits, and attention to unimportant aspects of the problem. In practice at many activities, anticipatory and perseverative errors are made and have to be overcome before the criterion can be attained. Such associations are usually less well formed than the primary ones and, as a result of this fact, are forgotten sooner, thus permitting the learner to return to practice with fewer inhibiting conditions to handicap the performance following rest.

In the form of the hypothesis adopted by Hull, inhibition is treated as an operationally defined construct, and the inhibitory processes may presumably express themselves in some of the ways described in the last paragraph or in other ways not specified. The important thing is that they operate to retard learning and that, by definition, they are forgotten faster than the excitatory processes.

The differential forgetting hypothesis fits, and is supported by, a wide range of the facts of reminiscence and distributed practice. Some of these facts are reviewed below.

The frequently obtained result that the wider forms of distribution are the more effective would be predicted from it in the case of activities at which important amounts of learning of correct and conflicting responses can occur during a short period of practice. This is because the interpolation of a rest interval after short practice periods gives nearly maximal opportunity for differential forgetting.

According to a differential forgetting theory, perceptual-motor activities and disparate verbal lists should show, as they do, greater differences under spaced versus unspaced practice than should poetry or prose, because the number of conflicting responses is usually greater in the former. On the other hand, there is some evidence to indicate that more reminiscence occurs in the learning of poetry or prose than in the learning of nonsense material. This

finding may, however, be due to methodological inadequacies in the experiments in question.

If we adopt the theory of serial position effects according to which the bow-shaped serial position curve is a function of intra-serial interference, then Hovland's (1938c, 1940b) findings that the greatest differences between the serial position curves obtained under massed and distributed practice come in the central part of the list, and that this difference increases with increasing length of list, support the hypothesis. Likewise, when Hovland (1940a) plots errors on the first recall trial twenty-four hours after learning by massed practice, he finds a flat serial position curve which implies that errors have dropped out (Figure 25). A deeply bowed curve

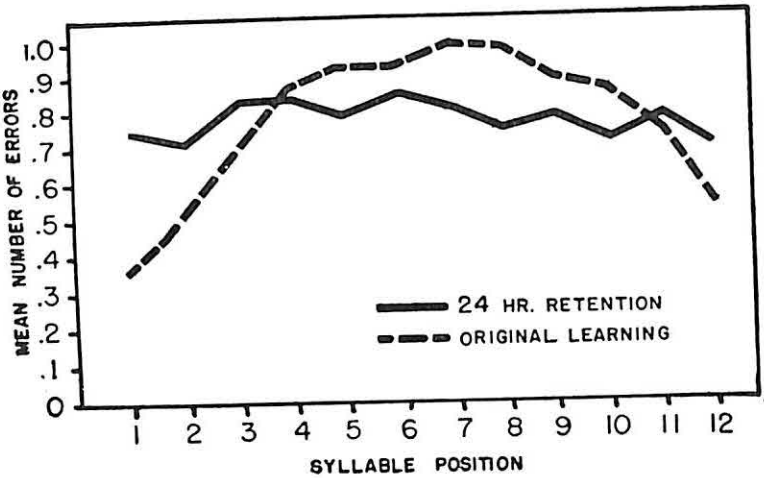

FIG. 25. MEAN ERRORS AT EACH SERIAL POSITION IN RECALL 24 HOURS AFTER REACHING A CRITERION OF ONE PERFECT TRIAL BY MASSED PRACTICE, AND AT A COMPARABLE STAGE OF PERFORMANCE IN ORIGINAL LEARNING
(From Hovland, *J. exp. Psychol.*, 1940a, 26, p. 577)

reappears on the second relearning trial, indicating a rapid building up or relearning of interference effects. The interpretation of this latter phenomenon is doubtful, however, since it is not clear why the weaker associations should profit more from a new repetition than should the stronger ones.

A comparison of the serial position curves for older and younger

series of nonsense syllables gives data which may be interpreted in terms of the differential forgetting hypothesis. The older series have fewer mean errors in the central positions than do the younger, which implies that the longer interval has been accompanied by a greater dissipation of intraserial interference (A. C. Youtz, 1941). This discovery leads Youtz to reformulate Jost's first hypothesis, as follows: "*Of two series of associations which are overtly remembered to the same degree, the one exhibiting the most extensive dissipation of intralist inhibition will profit more on a new repetition.*" This formulation brings together the phenomena of Jost's law, reminiscence, and distribution under the single hypothesis of the dissipation of inhibition with the passage of time.

A direct attack upon the problem of differential forgetting has been made by McGourty (1940) as a part of the investigations already described in the discussion of serial position curves. When the items at the four intermediate positions of the list were constructed with a high degree of formal similarity to each other (as CZR, ZTC, TQZ, QVT), they were learned less rapidly than control lists in which formal similarity was randomly distributed, but a significantly larger number of them were recalled on both the first and the second relearning trials. Trials to relearn showed no comparable differences. These results may be interpreted to mean a forgetting of interfering associations. The two lists differed with respect to the composition of the middle four items, which in one list were constructed in a way known to produce interference from list to list. The fact that these four items were learned less rapidly must be referred to interference among them, and their recall to a greater degree is best interpreted as a faster dropping out of interfering and wrong associations than of correct ones.

The hypothesis that differential forgetting is the fundamental condition underlying reminiscence and the greater effectiveness of distributed practice is supported by a considerable body of fact and can account for a wide range of data. At the present time, it appears to be one of the most fruitful of the theories which attempt to explain these phenomena.

There is still much that needs to be known, however, before exact

predictions can be made from it. It does not tell us why any given length of practice period or of rest interval is more effective than other lengths, nor why optimal length of rest appears, in many cases, to be independent of amount of practice between rests. We may assume that this is due, in part, to the fact that for any specific material a given amount of practice is accompanied by the fixation of correct and incorrect associations adequate for differential forgetting to operate with a clear influence after a given interval. We need to know, however, the component responses, correct and incorrect, and what their rates of forgetting are. We also may ask the question, why are either the correct or the incorrect responses forgotten at all? As will be shown in a later chapter, forgetting itself may be attributed, in part, to other interference effects. We are thus faced with the situation where one set of interferences interferes with, and thus removes, another set of interferences. A deduction from this state of affairs would lead to the conclusion that small amounts of practice at some extraneous interfering activity during the rest period(s) of reminiscence or distribution experiments should enhance recall beyond the effects of mere inactivity.

We also need to know whether remote associations are related to time interval in a way to fit the expectation that they should be one of the forms of interference lost during the rest periods. As we have seen, the evidence is not clear on this point. Why, also, if interference builds up as rapidly as it does in the experiments by Hovland and McGourty, are practice periods of more than a few trials ever effective in distribution schemes?[20] This is not a too difficult question for the hypothesis because small amounts of practice per session have usually been found to be optimal with verbal materials. The other problems raised in these paragraphs, and analogous ones which have not been discussed, are not necessarily fatal to the hypothesis, but are certainly matters upon which further knowledge is desirable. Without this knowledge, the differential forgetting hypothesis cannot be adequately evaluated.

[20] Cf. Hovland's (1940a) discussion of the assumptions which would be required to make the hypothesis of differential forgetting account for his results that retention after distributed practice is higher than after massed practice.

The Present Status of the Problem

The experiments on reminiscence and the distribution of practice have found that the introduction of time intervals between practice periods yields more rapid learning, under a wide range of conditions, than does continuous practice. An adequate theory must state the sufficient conditions which determine this facilitation. Each of the hypotheses reviewed in this chapter states conditions which may influence some of the results. However, at the present time, no one of these hypotheses can be considered to be entirely adequate. It is probable that no single set of conditions underlies all of the results, but that a combination of two or more hypotheses is necessary. Rehearsal, refractory phase, perseveration, and motivation seem, at present, to be only occasional determiners and to lack any great generality, although subsequent research may further strengthen the importance of perseveration. Reactive inhibition, particularly in connection with motor learning studies, and the differential forgetting hypotheses appear, at present, to offer the best-supported account of the phenomena and to have the most general explanatory significance.

REFERENCES

Ammons, R. B. Acquisition of motor skill: I. Quantitative analysis and theoretical formulation. *Psychol. Rev.*, 1947a, 54, 263-281.

Ammons, R. B. Acquisition of motor skill: II. Rotary pursuit performance with continuous practice before and after a single rest. *J. exp. Psychol.*, 1947b, 37, 393-411.

Ballard, P. B. Obliviscence and reminiscence. *Brit. J. Psychol., Monogr. Suppl.*, 1913, 1, No. 2.

Bell, H. M. Rest pauses in motor learning as related to Snoddy's hypothesis of mental growth. *Psychol. Monogr.*, 1942, 54, No. 1.

Bills, A. G., and Brown, C. The quantitative set. *J. exp. Psychol.*, 1929, 12, 301-323.

Book, W. F. *Learning to typewrite, with a discussion of the psychology and pedagogy of skill.* New York: Gregg, 1925.

Brown, W. To what extent is memory measured by a single recall? *J. exp. Psychol.*, 1923, 49, 191-196.

Bunch, M. E. The measurement of reminiscence. *Psychol. Rev.*, 1938, 45, 525-531.

Buxton, C. E. Reminiscence in the acquisition of skill. *Psychol Rev.*, 1942, 49, 191-196.

Buxton, C. E. Level of mastery and reminiscence in pursuit learning. *J. exp. Psychol.*, 1943a, 32, 176-180.

Buxton, C. E. The status of research in reminiscence. *Psychol. Bull.*, 1943b, 40, 313-340.

Buxton, C. E., and Bakan, M. B. Correction vs. non-correction learning techniques as related to reminiscence in serial anticipation learning. *J. exp. Psychol.*, 1949, 39, 338-341.

Buxton, C. E., and Ross, H. V. Relationship between reminiscence and type of learning technique in serial anticipation learning. *J. exp. Psychol.*, 1949, 39, 41-46.

Cain, L. F., and Willey, R. de V. The effect of spaced learning on the curve of retention. *J. exp. Psychol.*, 1939, 25, 209-214.

Carr, H. A. Distribution of effort. *Psychol. Bull.*, 1919, 16, 26-28.

Cook, T. W. Massed and distributed practice in puzzle solving. *Psychol. Rev.*, 1934, 41, 330-355.

Cook, T. W. Distribution of practice and size of maze pattern. *Brit. J. Psychol.*, 1936-1937, 27, 303-312.

Cummins, R. A. Improvement and the distribution of practice. *Teach. Coll. Contr. Educ.*, 1919, No. 97.

Dodge, R. Note on Professor Thorndike's experiment. *Psychol. Rev.*, 1927, 34, 237-240.

Doré, L. R., and Hilgard, E. R. Spaced practice and the maturation hypothesis. *J. Psychol.*, 1937, 4, 245-259.

Doré, L. R., and Hilgard, E. R. Spaced practice as a test of Snoddy's two processes in mental growth. *J. Psychol.*, 1938, 23, 359-374.

Duncan, C. P. The retroactive effect of electroshock on learning. *J. comp. physiol. Psychol.*, 1949, 42, 32-44.

Easley, H. The curve of forgetting and the distribution of practice. *J. educ. Psychol.*, 1937, 28, 474-478.

Ebbinghaus, H. *Über das Gedächtniss: Untersuchungen zur experimentellen Psychologie.* Leipzig: Duncker and Humblot, 1885. (Trans. as *Memory: a contribution to experimental psychology* by H. A. Ruger and C. E. Bussenius. New York: Teachers College, Columbia Univ., 1913.)

Ericksen, S. C. Variability of attack in massed and distributed practice. *J. exp. Psychol.*, 1942, 31, 339-345.

Gentry, J. R. Immediate effects of interpolated rest periods on learned performance. *Teach. Coll. Contr. Educ.*, 1940, No. 799.

Gordon, K. Class results with spaced and unspaced memorizing. *J. exp. Psychol.*, 1925, 8, 337-343.

Gray, S. The influence of methodology upon the measurement of reminiscence. *J. exp. Psychol.*, 1940, 27, 37-44.

Gundlach, R., Rothschild, D. A., and Young, P. T. A test and analysis of "set." *J. exp. Psychol.*, 1927, 10, 247-280.

Gustafson, L. M., and Irion, A. L. Reminiscence in bilateral transfer. Unpublished paper read at MPA, Chicago, 1951.

Hardy, M. C. The effect of distribution of practice in learning a stylus maze. *J. comp. Psychol.*, 1930, 10, 85-96.

Heathers, G. L. The avoidance of repetition of a maze reaction in the rat as a function of the time interval between trials. *J. Psychol.*, 1940, 10, 359-380.

Hilgard, E. R., and Smith, M. B. Distributed practice in motor learning: Score changes within and between daily sessions. *J. exp. Psychol.*, 1942, 30, 136-146.

Hovland, C. I. Experimental studies in rote-learning theory. I. Reminiscence

following learning by massed and distributed practice. *J. exp. Psychol.*, 1938a, 22, 201-224.

Hovland, C. I. Experimental studies in rote-learning theory. II. Reminiscence with varying speeds of syllable presentation. *J. exp. Psychol.*, 1938b, 22, 338-353.

Hovland, C. I. Experimental studies in rote-learning theory. III. Distribution of practice with varying speeds of syllable presentation. *J. exp. Psychol.*, 1938c, 23, 172-190.

Hovland, C. I. Experimental studies in rote-learning theory. IV. Comparison of reminiscence in serial and paired associate learning. *J. exp. Psychol.*, 1939a, 24, 466-484.

Hovland, C. I. Experimental studies in rote-learning theory. V. Comparison of distribution of practice in serial and paired associate learning. *J. exp. Psychol.*, 1939b, 25, 622-633.

Hovland, C. I. Experimental studies in rote-learning theory. VI. Comparison of retention following learning to the same criterion by massed and distributed practice. *J. exp. Psychol.*, 1940a, 26, 568-587.

Hovland, C. I. Experimental studies in rote-learning theory. VII. Distribution of practice with varying lengths of list. *J. exp. Psychol.*, 1940b, 27, 271-284.

Hovland, C. I. Experimental studies in rote-learning theory. VIII. Distributed practice of paired associates with varying rates of presentation. *J. exp. Psychol.*, 1949, 39, 714-718.

Huguenin, C. Reviviscence Paradoxale. *Arch. d. Psychol.*, 1914, 14, 379-383.

Hull, C. L. Simple trial and error learning: a study in psychological theory. *Psychol. Rev.*, 1930, 37, 241-256.

Hull, C. L. The conflicting psychologies of learning—a way out. *Psychol. Rev.*, 1935, 42, 491-516.

Hull, C. L. Simple trial and error learning—an empirical investigation. *J. comp. Psychol.*, 1939, 27, 233-258.

Hull, C. L., et. al. *Mathematico-deductive theory of rote learning: a study in scientific methodology*. New Haven: Yale Univ. Press, 1940.

Hull, C. L. *Principles of behavior*. New York: D. Appleton-Century, 1943.

Humphreys, L. G. The factor of time in pursuit rotor learning. *J. Psychol.*, 1937, 3, 429-436.

Hunter, W. S. The auditory sensitivity of the white rat. *J. Anim. Behav.*, 1914, 4, 215-222.

Irion, A. L. The relation of "set" to retention. *Psychol. Rev.*, 1948, 55, 336-341.

Irion, A. L. Reminiscence in pursuit-rotor learning as a function of length of rest and of amount of pre-rest practice. *J. exp. Psychol.*, 1949a, 39, 492-499.

Irion, A. L. Retention and warming-up effects in paired-associate learning. *J. exp. Psychol.*, 1949b, 39, 669-675.

Johnson, L. M. The relative effect of a time interval upon learning and retention. *J. exp. Psychol.*, 1939, 24, 169-179.

Jost, A. Die Assoziationsfestigkeit in Ihrer Abhängigheit von der Verteilung der Wiederholungen. Z. *Psychol.*, 1897, 14, 436-472.

Kimble, G. A. An experimental test of a two-factor theory of inhibition. *J. exp. Psychol.*, 1949a, 39, 15-23.

Kimble, G. A. A further analysis of the variables in cyclical motor learning. *J. exp. Psychol.*, 1949b, 39, 332-337.

Kimble, G. A. Performance and reminiscence in motor learning as a function of the degree of distribution of practice. *J. exp. Psychol.*, 1949c, 39, 500-510.

Kimble, G. A., and Bilodeau, E. A. Work and rest as variables in cyclical motor learning. *J. exp. Psychol.*, 1949, 39, 150-157.

Kimble, G. A., and Horenstein, B. R. Reminiscence in motor learning as a function of length of interpolated rest. *J. exp. Psychol.*, 1948, 38, 239-244.

Lashley, K. S. A causal factor in relation of the distribution of practice to the rate of learning. *J. Anim. Behav.*, 1917, 7, 139-142.

Lashley, K. S. A simple maze; with data on the relation of the distribution of practice to the rate of learning. *Psychobiol.*, 1918, 1, 353-367.

Leuba, J. H., and Hyde, W. An experiment on learning to make hand movements. *Psychol. Rev.*, 1905, 12, 351-369.

Lorge, I. The influence of regularly interpolated time intervals upon subsequent learning. *Teach. Coll. Contr. Educ.*, 1930, No. 438.

Lyon, D. O. The relation of length of material to time taken for learning and optimum distribution of time. *J. educ. Psychol.*, 1914, 5, 1-9; 85-91; 155-163.

Magdsick, W. K. The curve of retention of an incompletely learned problem in albino rats at various age levels. *J. Psychol.*, 1936, 2, 25-48.

Mayer, B. A., and Stone, C. P. The relative efficiency of distributed and massed practice in maze learning by young and adult albino rats. *J. genet. Psychol.*, 1931, 39, 28-48.

McClatchy, V. R. The optimal position of a rest period in learning. *J. exp. Psychol.*, 1925, 8, 251-277.

McClelland, D. C. Studies in serial verbal discrimination learning. I. Reminiscence with two speeds of pair presentation. *J. exp. Psychol.*, 1942, 31, 44-56.

McClelland, D. C. Studies in serial verbal discrimination learning. III. The influence of difficulty on reminiscence in responses to right and wrong words. *J. exp. Psychol.*, 1943, 32, 235-246.

McGeoch, G. O. The conditions of reminiscence. *Amer. J. Psychol.*, 1935, 47, 65-87.

McGourty, M. C. Serial position effects in learning as a function of interfering associations. Iowa City: M. A. Thesis deposited in State University of Iowa Library, 1940.

Melton, A. W. The effect of rest pauses on the acquisition of the pursuitmeter habit. *Psychol. Bull.*, 1941, 38, 719. (Abstract.)

Melton, A. W., and Stone, G. R. The retention of serial lists of adjectives over short time intervals with varying rates of presentation. *J. exp. Psychol.*, 1942, 30, 295-310.

Miller, N. E., and Dollard, J. *Social learning and imitation.* New Haven: Yale Univ. Press, 1941.

Mowrer, O. H. Anxiety-reduction and learning. *J. exp. Psychol.*, 1940, 27, 497-516.

Mowrer, O. H., and Jones, H. M. Extinction and behavior variability as functions of effortfulness of task. *J. exp. Psychol.*, 1943, 33, 369-386.

Müller, G. E., and Pilzecker, A. Experimentelle Beiträge zur Lehre vom Gedächtniss. *Z. Psychol.*, 1900, Ergbd. 1.

Noble, C. E. Absence of reminiscence in the serial rote learning of adjectives. *J. exp. Psychol.*, 1950, 40, 622-631.

Patten, E. F. The influence of distribution of repetitions on certain rote learning phenomena. *J. Psychol.*, 1938, 5, 359-374.

Pyle, W. H. Economical learning. *J. educ. Psychol.*, 1913, 4, 148-158.

Renshaw, S., and Schwarzbek, W. C. The dependence of the form of the pursuitmeter learning function on the length of the interpractice rests. I. Experimental. *J. gen. Psychol.*, 1938, 18, 3-16.

Reyna, L. J. An experimental study of work inhibition in motor learning. Iowa City: M. A. Thesis deposited in the State University of Iowa Library, 1944.

Robinson, E. S. The relative efficacy of distributed and concentrated study in memorizing. *J. exp. Psychol.*, 1921, 4, 327-343.

Robinson, E. S. Work of the integrated organism. Chap. 12 in C. Murchison (Ed.), *Handbook of general experimental psychology*. Worcester: Clark Univ. Press, 1934.

Rohrer, J. H. Factors influencing the occurrence of reminiscence: Attempted formal rehearsal during the interpolated period. *J. exp. Psychol.*, 1949, 39, 484-491.

Snoddy, G. S. *Evidence for two opposed processes in mental growth*. Lancaster, Pa.: Science Press, 1935.

Snoddy, G. S. A reply to Doré and Hilgard. *J. exp. Psychol.*, 1938, 23, 375-383.

Solomon, R. L. The influence of work on behavior. *Psychol. Bull.*, 1948, 45, 1-40.

Spight, J. B. Day and night intervals and the distribution of practice. *J. exp. Psychol.*, 1928, 11, 397-398.

Telford, W. C. The refractory phase of voluntary and associative processes. *J. exp. Psychol.*, 1931, 14, 1-36.

Thorndike, E. L. *Educational psychology*. New York: Teachers College, Columbia Univ., 1914, Vol. 3.

Thorndike, E. L. The refractory period in associative processes. *Psychol. Rev.*, 1927, 34, 234-236.

Thune, L. E. The effect of different types of preliminary activities on subsequent learning of paired-associate material. *J. exp. Psychol.*, 1950, 40, 423-438.

Travis, R. C. The effect of the length of the rest period on motor learning. *J. Psychol.*, 1937, 3, 189-194.

Ulrich, J. L. The distribution of effort in learning in the white rat. *Behav. Monogr.*, 1915, 2, No. 10.

Von Förster, H. *Das Gedächtniss: eine quantenphysikalische Untersuchung*. Vienna: Franz Deuticke, 1948.

Ward, L. B. Reminiscence and rote learning. *Psychol. Monogr.*, 1937, 49, No. 220.

Warden, C. J. The distribution of practice in animal learning. *Comp. Psychol. Monogr.*, 1923, 1, No. 3.

Wheeler, R. H. *The science of psychology: an introductory study*. New York: Crowell, 1929.

Wheeler, R. H., and Perkins, F. T. *Principles of mental development: a textbook in educational psychology*. New York: Crowell, 1932.

Williams, O. A study of the phenomenon of reminiscence. *J. exp. Psychol.*, 1926, 9, 368-387.

Withey, S., Buxton, C. E., and Elkin, A. Control of rest interval activities in experiments on reminiscence in serial verbal learning. *J. exp. Psychol.*, 1949, 39, 173-176.

Wright, S. T. H., and Taylor, D. W. Distributed practice in verbal learning and the maturation hypothesis. *J. exp. Psychol.*, 1949, 39, 527-531.

Youtz, A. C. An experimental evaluation of Jost's laws. *Psychol. Monogr.*, 1941, 53, No. 238.

VII

LEARNING AS A FUNCTION OF MOTIVE-INCENTIVE CONDITIONS

INTRODUCTION

Motivation is an inferred concept or intervening variable which may be defined in a number of ways. It may be inferred from the behavior of the individual or from certain events which happen to him, or both. In the study of motivation, the difficulty is immediately encountered that this term has had, for different investigators, a great variety of meanings. In the context of some experiments, this term has implied only some native or primary drive state resulting from some condition of physiological imbalance. For other investigators, the influence of learned drives, such as fear, has been of greater importance. Still other experiments have been concerned with the enormously complex motivational structure of the human adult. In view of the fact that complete continuity along this scale of complexity has yet to be established, it is, perhaps, unwise to attempt to define this concept too specifically. On the other hand, a general definition will serve to clarify a discussion of motivational effects in learning and will also help the student to understand the subsequent chapter on the Law of Effect.

For purposes of discussing learning, we may say that *a motive or motivating condition is any condition of the individual which initiates and sustains his behavior, orients him toward the practice of a given task, and which defines the adequacy of his activities and the completion of the task.*[1] Under this definition we should expect

[1] The general problem of the systematic psychology of motivation lies outside the scope of this book. The definition given here is a working formulation which fits fairly well the problem of motivation in learning. The reader will find discussions of many special and general problems in this area in Diserens and Vaughn (1931), Stone (1934), Young (1936), Bird (1940), Underwood

to find motivated individuals to be more active than unmotivated ones, and, in fact, this increased activity level is often used as a measure of motivational strength. Similarly, we should expect motivated individuals to be oriented toward particular objects and activities, and, correspondingly, to neglect other activities. In the same manner, we would expect that, once a motive has been satisfied in the course of such increased and selective activity, the activity level will decline and the restriction of behavioral variability will become less marked (cf. Melton, 1941). As we shall see, not all of these characteristics invariably accompany a state of motivation, although they usually do. In particular, orientation of behavior may be lacking because this depends upon previous experience with the motive-incentive conditions concerned. Individuals usually become oriented toward particular activities, goals, or incentives, only when these have been associated with motive reduction in the past. On the other hand, following the first period of life, completely novel incentives are rarely experienced.

A word may be said, perhaps, concerning the use of the term, *incentive*. Motives implicitly point beyond themselves to objects or conditions which satisfy them by diminishing or removing them. Satisfaction means only this diminution or removal. *An object or condition which is reacted to as satisfying or potentially satisfying to a motive is called an incentive.* Although motive and incentive are logically separate, they are so interrelated in both experimental and practical situations that it is useful to speak of them in combination as *motive-incentive* conditions.[2]

(1949), and Miller (1951). Theoretical treatments of motivation may be found in Carr (1925), Lewin (1935), Sherif and Cantril (1947), Miller (1951), and in a series of papers and books by Hull. Of these, his paper on knowledge and purpose (1930), his paper on goal attraction and directing ideas (1931), and his *Principles of Behavior* (1943) are possibly the most significant.

[2] The separability of incentive and motive will become clear upon reflection. The subject may be motivated, but no incentive may be present. Conversely, the incentive may be present, but may appeal to no motive then present in the individual. As we have seen, moreover, certain objects which may be adequate to satisfy a motive may not be reacted to as an incentive because of a lack of experience with them (cf. Kendler, 1942a).

Primary Motivation

The terms, motive, incentive, and learning are almost inextricably intertwined, and separation of their meanings is correspondingly difficult. One reason for this difficulty is that, while motivation is a necessary condition of the learning process, many motives are, themselves, the product of learning. As we have already noted, a similar mechanism exists by means of which the attracting value of certain incentive objects is established. This interrelation points to the fact that certain motives are unlearned and serve as an ultimate basis for all subsequent learning of other motives. Such unlearned motives are termed *primary motives* or *primary drives*. They result from physiological imbalances which occur when certain essential substances, such as food or water, are needed. It is to be noted that not all conditions of need result in drive behavior, but only those conditions of need which have been associated with problems of survival and reproduction in the history of the species. Thus, need for oxygen does not produce a state of motivation while an excess of carbon dioxide does. This is undoubtedly because the normal habitat of man contains very few situations where oxygen lack is not correlated with increased carbon dioxide content. Since the invention of the airplane, it is possible for men to suffer from anoxia without suffering from a corresponding increase in carbon dioxide. Under such circumstances, unconsciousness and death may ensue without conscious discomfort and without the heightened activity level which is characteristic of drive states becoming manifest. For the commonly recurring needs for food and water, however, primary drive mechanisms exist. An animal or man deprived of these substances for any considerable period of time will exhibit an increase in activity level and an increased orientation toward those stimuli and activities associated with the appropriate incentives in the past. This activity and orientation will continue until the drives are removed or physiological weakness overcomes the individual. Two primary drive states deserve special consideration because of their slightly atypical characteristics. These are sex and pain. Sexual motivation is of obvious importance in the matter of species sur-

vival,[3] and it is not surprising to find powerful sexual motives over a very wide range of species. This motivation betrays its presence not only in terms of sexual behavior, but also in terms of the general activity level. Thus, in the female rat, activity level is correlated with the oestrual cycle. In the male rat, great reduction in activity level occurs almost immediately following castration.

Pain, as a drive, differs from other primary drives in that the need associated with it may arise suddenly as a result of external stimulation. The resulting painful sensations have a high motivational value. The biological utility of this mechanism is apparent since it is by this means that many threats to survival are terminated in the present and avoided in the future.

All of the primary drive states are accompanied by marked sensory phenomena. The pangs of hunger, the sensations of thirst, and the voluptuous feelings of sexual excitation are examples of this. The painful sensations resulting from noxious stimulation are even more marked. So pronounced is the afferent receptor discharge component of drive states that the question may be asked, is drive anything more than stimulation and is drive satisfaction anything more than the reduction of this stimulation? (Cf. Miller and Dollard, 1941.) It is impossible to give an unequivocal answer to this question on the basis of existing information. It should be noted, however, that drive stimulation plays an important role in learning and performance in a number of ways. For one thing, it is undoubtedly responsible, at least in part, for the heightened activity level which accompanies drive states. This heightened activity level, in turn, is an important condition of practice because it makes more likely the discovery of adequate, i.e., motive-satisfying, modes of response. Heightened activity level, furthermore, serves to energize ongoing habitual behavior, and thus to increase the level of performance, at least in many cases. Not less important is the fact that drive stimuli are discriminably different from each other and, consequently, may

[3] It is perhaps unnecessary to note that it is reproduction rather than survival which is the basic factor in evolution and the survival of species. On the other hand, it is clear that no person ever had an ancestor, however remote, who did not survive beyond infancy.

serve as conditioned stimuli for the elicitation of habitual ways of responding. It is because of this that we engage in the habit of going to the restaurant when we are hungry and to the water cooler when we are thirsty, and that we almost never make mistakes in such matters (cf. Hull, 1933, and Leeper, 1935).[4]

Learned Motives[5]

Since primary drives often involve an elaborate response mechanism, it is possible that the operation of that response mechanism may become attached, through learning, to stimuli which are originally powerless in this respect. Thus, the elaborate responses of the body to noxious stimulation (Cannon, 1915) may become conditioned to stimuli which accompany that stimulation. Such conditioning is, of course, only partial. The painful sensations aroused by the noxious stimulus are not learned, but it seems evident from the work on emotional conditioning that other aspects of the response do become learned.[6] These learned responses, commonly referred to as fear or anxiety responses, evidently play a very significant motivational role in the determination of man's behavior. Painful stimulation is, thus, the unconditioned stimulus for the learning of fear (cf. Miller and Dollard, 1941) and there is the distinct possibility that "higher order conditioning" of fear responses may occur. The opportunity for acquiring such fear motivation is obviously great.

[4] This discriminability of drives also serves as the basis for eliciting many other, less socially valuable, instrumental responses. Thus, discriminable amounts of fear or anxiety motivation may elicit characteristic protective habits of a psychoneurotic character. It may also be noted that confusion of motives does, upon occasion, occur.

[5] The following discussion is, of necessity, somewhat speculative. This is because, at the present time, little experimental evidence exists with respect to the manner and course of development of the learned motivational structure of man.

[6] There is a large literature upon this subject using both human and animal subjects. Pioneer experimentation in this field was conducted by Watson and Rayner (1920). Later papers have appeared under the general headings of emotional conditioning, conditioning of the galvanic skin response, and the learning of anxiety. It is not possible to list all of these studies, although the reader interested in this subject would do well to consult the papers of Mowrer (1939), Mowrer and Lamoreaux (1942), Miller (1948), and Farber (1948). Dollard and Miller (1950) contains an excellent discussion of this matter.

On the other hand, there is no reason to expect that a large number of *different* motives may be acquired in this way. Certainly the number of acquired motives cannot well exceed the number of primary motives of which they are a partial reproduction. It is questionable, moreover, whether all primary drive states can have secondary motives based upon them in this way. Concerning acquired fear and acquired sexual motivation there can be no doubt. The contractions of the stomach associated with hunger may, very probably, be learned, and certainly secretory responses connected with digestion can be (cf. Pavlov, 1927; Hull, 1934). Concerning the acquisition of other partial reproductions of primary drive states much less is known. The existence of other secondary motives acquired in this way must be questioned until further information is available.

It would appear, however, that while the number of secondary drives may be small, the number of stimuli which come to elicit them may be enormous. As the individual develops, the organization of these stimulus-response relations tends to become increasingly complex, and eventually includes reactions to symbols of such complicated things as class status and individual role. It is probable that, very early in life, the individual acquires the tendency to react with fear or anxiety to stimuli which conflict with his own verbal description of himself, that is, his role. Much of the work on success and failure, rivalry, and ego-involvement can be interpreted in terms of such fear and the complicated reactions associated with that motivational state.

Other Derived Motivational States

All of these motivational states, primary and secondary, involve a heightened activity level of the individual and a demonstrable increase in the level of stimulation. A number of other situations produce these characteristic effects, also. For example, a great deal of man's behavior is oriented toward particular incentive objects. Such behavior tends to form an integrated chain of responses, the drive state and the fractional anticipatory goal responses serving as

the integrating mechanisms (cf. Hull, 1930, 1931).[7] When an individual, behaving in this purposive or goal-oriented manner, is prevented from gaining the appropriate incentive object, we may speak of *frustration.* Frustration, in turn, typically results in an increased stimulation-activity level of the individual. There are, of course, many situations in the life of every individual which can be described as frustrating. Some of these frustrations occur because the appropriate incentive object is not forthcoming. The diner who has ordered his lunch becomes increasingly frustrated as the minutes roll by without the appearance of food. The child who has successfully employed certain techniques for gaining social approval and recognition in grade school (*i.e.*, bringing reptiles to school, being the best marble player, showing off, etc.) is frustrated when these same techniques fail to win recognition or cause rebuffs in high school. Frustration may result, too, from a conflict of reaction systems within the individual which prevents effective action toward either of two or more incentive objects.[8] The increase in muscular tension and stimulation level associated with frustration is, in turn, motivating in its own right and may, moreover, be the occasion for the elicitation of conditioned fear responses. Thus, the soldier who is frustrated by his superior officer not only suffers from increased motivation as a consequence of this frustration, but also suffers from increased fear motivation resulting from the conflict between his desire to strike the officer and his fear of the consequences for such an action on his part.

It is also interesting to note that if activity level is increased directly, as by having the subject manipulate a dynamometer, that motivation-like effects may be obtained. Since this is discussed in more detail in a later section of this chapter, no more will be said of it here.

Another motivation-like effect which can be obtained by direct

[7] The factor of secondary reinforcement in this integration may be of great significance also (Hull, 1943; Spence, 1947).

[8] This type of conflict situation has been extensively considered by Guthrie (1938). Conflicts based upon conditioned fear as the negative component have been discussed by Miller (1944) and Underwood (1949) (cf. also, Dollard, Doob, Miller, Mowrer, and Sears, 1939).

intervention of the experimenter is concerned with the subject's *set to respond*. Much of human learning, both in the laboratory and in everyday life, is done in response to instructions of some kind and without explicitly provided formal motives or incentives. The chief effect of the instruction is to arouse or produce a set which outlasts the instruction and influences behavior in the direction intended—that is, directs it. Set appears to have, then, the orienting and directing property of other motives. As in the other cases we have noted, this directing property is based upon the past experiences of the subjects. The concept of set is arrived at inferentially or by the subject's report and is not directly measured in the learning experiment. It is, however, controllable to a certain extent through the giving of instructions of various kinds. Although suffering from a certain vagueness of definition, set is nevertheless a useful concept, and the existence of the phenomena referred to it is undoubted. (Cf. J. J. Gibson, 1941.) For the purposes of experiments in human learning it can be regarded as being continuous with the concept of motive.

It is a commonplace fact of experimental procedure that the experimenter must tell the subject what is expected of him in the experimental setting, must orient him toward what he is to practice, and in some degree toward how he is to practice it.[9] Thus, one may say, "Learn by the anticipation method," and then describe the method, or "You are at the starting point of a maze; find your way to the goal" (with further description of a maze), and so on. Instructions vary in specificity of detail and in completeness according to the problem and the character of the experiment. Given the instructions, the subject in front of a memory drum is set to read each word as it appears in the window and to connect the items so that he can anticipate each one; the subject at the starting point of a maze is set to find his way to the point at which the experimenter will say

[9] There are, of course, exceptions to this. In the conditioning experiments, particularly those with very young children or with adult aments, few or no instructions may be given. Similarly, it is occasionally desirable for the experimenter to place the subject in an experimental situation without giving him instructions of any kind. In these cases, of course, other motivational conditions must obtain if learning is to occur.

"Goal"; the subject faced with a rational problem is set to attempt a solution. It is a problem of the first importance to measure the influence that variations in instructions and in the resulting set of the subject have upon learning.

It should be observed that the formal instructions are not the only ones instrumental in establishing a set in the subject. Stimuli of many kinds may do it: the tone of the experimenter's voice, or some unintended interpretation of the formal instructions. Further, the subject may instruct himself and thereby orient himself in directions which may or may not coincide with those intended by the experimenter. There are, thus, three varieties of instructions: (a) the *formal* ones of the experimental procedure; (b) the *incidental, casual,* or *occasional* ones aroused by the situation and without the intent of the experimenter; and (c) the instructions the subject gives himself—that is, *self instructions*. In a later paragraph, we shall examine some of the effects of these instructional sets upon the learning process.

The Relation of Secondary Reinforcement to Motivation

When we consider human motivation loosely, we often speak of such "motives" as a desire for money, a desire for social status, or a desire to go to the movies. Actually, such motives as these may be extremely complicated and may be made up, in part, of components which are distinctly non-motivational in character. Thus, a desire for social status may be, in part, a complexly stimulated fear of social rejection and, in part, a tendency to react positively toward certain symbols of social approval. It is this tendency to react positively toward certain symbols that we speak of as secondary reinforcement. Since the characteristics of secondary reinforcement are discussed elsewhere in this book (Chapter VII), we shall give only a brief statement of its major characteristics here. Certain stimuli which are associated in time, upon a number of occasions, with primary rewards (food, water, etc.), as a result of this association acquire the capacity to reward or reinforce habits even in the absence of the primary rewards and the primary motives which

were originally present. For example, Wolfe's (1936) chimpanzees, who associated food with tokens, would later solve problems in order to obtain such tokens. A great many stimuli acquire secondary reward value in the life history of every individual. For the infant, stimuli which are consistently associated with feeding or comforting take on such value. Not only the physical objects (nursing bottles, etc.) associated with these events, but also the persons and the activities of the persons involved, acquire secondary reward value. Thus, the presence and reactions of the mother or nurse become strong secondary reinforcing stimuli to the average child. The child will then tend to learn acts which are consistently followed by such secondary reinforcing stimuli. Many of the "attention-getting" habits of childhood are learned on this basis. Because the individual learns to react positively to such stimuli and because he learns habits which achieve such stimulation, we may say, loosely, that the individual has a "desire" for these things. Thus, the individual learns to respond to coins and currency in much the same fashion (although in a more complicated way) that the chimpanzee learns to respond to the tokens. Furthermore, once habits are established which lead toward symbolic rewards, the individual begins to anticipate the occurrence of these incentives. Blocking of his activity or the removal of these learned incentives will then produce frustration and the heightened motivational level associated with that state.

THE EFFECT OF MOTIVATION UPON LEARNING AND PERFORMANCE

There are several problems which must be examined in connection with the effect of motivation upon learning and performance. The first of these concerns the effect of motivational changes upon performance when habit remains constant. That is to say, given a particular amount of practice on a given task, can the performance level of the subject be changed by increasing or decreasing his motivational level without affording him additional practice? A second problem concerns the effect of motivational level upon the acquisition process, itself. That is, does learning occur more readily

under some motivational conditions than under others, and can this differential learning be demonstrated under subsequent test conditions wherein motivational level is held constant for all subjects? In connection with this, we must also note the relationship between motivational level and the operation of the law of effect. A third general problem concerns the techniques which may be used for increasing and decreasing the motivational level of human subjects. In connection with this, we shall wish to examine a sample of the experimental results which have been obtained under different types of motive-incentive conditions.

The Effects of Motivation upon Performance

When learning has progressed to a measurable level, changes in the drive state of the learner may be reflected in immediate behavioral changes. These behavioral changes, moreover, occur when there is no opportunity for further learning to take place. It is important to note, however, that in the investigation of this problem, it is the drive rather than the incentive which must be manipulated by the experimenter. Changes in the magnitude of the incentive object cannot cause an influence upon behavior until at least one additional trial of practice has been conducted.[10] Studies on this problem have been conducted almost entirely with animal subjects. The reason for this, of course, lies in the relatively greater ease of manipulating motivational variables with animals.

In Hull's (1943) *Principles of Behavior,* this problem is given theoretical consideration. According to Hull's analysis, the intervening variables of habit (effective habit strength, $_{S}H_{R}$) and drive (D) combine in a multiplicative fashion to produce the intervening variable termed excitatory potential ($_{S}E_{R}$), which, other things

[10] As is noted elsewhere in the text, Hull (1943) assumes that magnitude of the incentive object has an influence upon learning whereas magnitude of the drive has an influence upon the performance of learned acts. In his later postulate system (1950) Hull assumes that both magnitude of incentive object and magnitude of drive influence the performance of learned acts and have no direct influence upon the acquisition process. For a number of reasons, Hull's earlier treatment appears to be more satisfactory.

being equal, determines the probability of occurrence, the amplitude, the latency, and the resistance to extinction of the activity involved. Thus, given a particular amount of habit strength, performance can be changed in characteristic ways by a manipulation of drive strength. It should also be noted that in this formulation Hull makes specific provision for the effects of irrelevant drives, that is, drives which are not concerned with the incentive toward which the activity is oriented.

Perhaps the best example of the effects of relevant drive changes upon performance is contained in the experiment by Perin (1942). In a portion of this experiment, Perin used four groups of forty animals (white rats) each. Each of these groups was given sixteen reinforcements in the Skinner box situation under a constant 23-hour hunger drive. The groups were then extinguished under different strengths of hunger motivation (1, 3, 16, or 23 hours of privation from food). Strength of performance, in this situation, can be measured by determining the amount of time necessary for extinction to occur, or by determining the number of bar-pressing responses necessary to achieve the criterion of extinction. Both of these measures, in Perin's experiment, indicated that, as hunger drive during extinction was increased, performance became stronger. Thus, using number of responses to reach the criterion of extinction as a measure, Perin found that a mean number of 9.10 responses occurred under 1 hour of deprivation, 12.30 responses under a 3-hour hunger drive, 24.50 responses under a 16-hour drive, and 32.90 responses when the animals had been deprived of food for 23 hours.[11]

Concerning the effects of manipulating irrelevant drive states, the results obtained by Webb (1949) are instructive. He employed five groups of animals (white rats). The animals in each group were

[11] It will be seen that there is a slight positive acceleration to this function. This is probably due to the fact that the animals were all trained under twenty-three hours of hunger but were extinguished under different intensities of hunger drive. Because the drive stimulus, in such cases, becomes a portion of the stimulus complex eliciting the response, the positive acceleration may be deduced from the fact of stimulus generalization. The student would do well to consult Perin's (1942) paper, which also discusses the findings of Williams (1938). Hull (1943) contains an excellent discussion of these data.

trained to perform a simple instrumental response to obtain food. At this time, all animals were operating under a 22-hour hunger drive. Following this, the animals were extinguished while satiated for food, but while suffering from various intensities of thirst drive. In group I, the animals were satiated for both food and water. In groups II, III, and V, the animals were satiated for food but suffered from 3, 12, and 22 hours of privation from water, respectively. In group V, animals were extinguished under the same motivational conditions that prevailed during training, that is, satiation for water and 22 hours of hunger. According to Hull's theory, this last group should show the greatest resistance to extinction because the stimulus complex remains unchanged between learning and extinction. On the other hand, as strength of the irrelevant drive increases (conditions I through IV) there should be a clear increase in the strength of performance as measured by resistance to extinction. This result was obtained. When median number of responses during extinction is plotted against hours of water deprivation, a nearly linear increasing function is obtained.[12]

One feature of Webb's experiment is that the relevant drive (hunger) was not present during the operation of the irrelevant need (thirst). In an experiment to determine the effects of combining relevant and irrelevant drives, Kendler (1945) found the irrelevant drive of thirst combined with the relevant drive of hunger (22 hours) to augment the performance of a lever-pressing response. This was true, however, only when moderate degrees of irrelevant thirst drive were employed. When a high level of thirst motivation was used in combination with hunger motivation, a lower level of performance was obtained than by means of the hunger motivation, alone. Siegel (1946) has obtained results which appear to corroborate this latter finding of Kendler's.

The problem of the interaction of motivational states, thus, is raised. It is possible that hunger operates to modify thirst motivation, and vice versa, and that the summation of drives does not follow any simple relationship. Furthermore, there is the distinct

[12] Webb fits a straight line to his data, the equation for which is: $y = 0.17x + 3.18$.

probability, based on physiological knowledge, that different pairs of drives summate and interact in different ways. Using a different combination of drives, Amsel (1950) failed to show a summation of pain and hunger drives, although he did demonstrate a summation effect when hunger and fear were combined. It is probable, as Amsel concludes, that the pain stimulus (electric shock) used in this experiment was too strong for summation to be demonstrated. This is an effect which could be obtained under the general plan of Hull's (1943) theoretical analysis.[13] When Amsel employed a combination of hunger and the weaker anxiety or fear drive (resulting from the shocks mentioned above), the expected summation was obtained.

Still another effect of motivation upon performance depends upon the conditioned stimulus function of the drive stimulus. Since all drives seem to have strong afferent components, the possibility that drive-produced stimuli may become components of the conditioned stimulus is apparent. Although much work remains to be done on this important problem, the general conclusion reached by Hull (1933) and Leeper (1935), that different drive stimuli may serve as a basis for discrimination learning, appears to be well founded. Kendler (1946, 1949a, b), in a series of studies, has considered this and related problems. His results would seem to indicate that, while drive stimuli do become conditioned stimuli, not all drive stimuli function in this way. According to this view, which Kendler terms the *selective principle of association of drive stimuli*, only relevant drive stimuli become conditioned stimuli for the elicitation of learned responses. That is to say, only those drive stimuli that are reduced as a result of reinforcement during training take on this function. For example, in one experiment, animals were trained in

[13] This follows from Hull's definition of the total effective drive state, which is given by the equation:

$$\text{total effective drive} = \frac{D + \dot{D}}{\dot{D} + M_D}$$

where D is the strength of the dominant primary drive, $\dot{D}$ is the combined strength of all irrelevant drives, and M_D is the maximum possible drive strength, i.e., 100 units (motes). It may be seen that when the value of D approaches the value of M_D, the effect of $\dot{D}$ will become minimized.

a T-maze under both hunger and thirst drives. One goal box of the T-maze was baited with water and the other with food. Following training in this situation, Kendler tested the animals under a single drive, either hunger or thirst. In this test, the thirsty animals responded appropriately 98 per cent of the time and the hungry animals responded appropriately 73 per cent of the time. Since drive stimuli for both hunger and thirst were present during training, these stimuli should have become associated with both responses in an approximately equal fashion. It would appear, then, that some selective principle is necessary to account for these findings.[14]

The Effects of Motivation upon Habit Acquisition

There are a number of ways in which motivation influences the acquisition process. In the first place, motivation is a necessary condition of practice. The heightened activity level that is characteristic of motivational states makes possible the "discovery" of correct ways of behaving. It is also worth noting that, in complex learning situations where the learning consists in large part of abandoning or extinguishing previously learned conflicting responses, high motivation may impede the learning process. This is because, as we have seen, increases in motivation are typically accompanied by an increased resistance to experimental extinction.[15]

The most important relationship between motivation and acquisition, however, is concerned with reinforcement or reward. As we shall see in Chapter VII, the empirical law of effect is a well-established principle of the learning process. Almost all formulations of the nature of reward include some statement regarding

[14] It would seem that Guthrie's theory is especially well adapted to handling these findings. According to this view, responses which occur when drive stimuli are reduced tend to become attached to those drive stimuli because they are the last responses to occur in the presence of those stimuli. Irrelevant drive stimuli, however, continue to operate and the responses learned to them are presently unlearned through the mechanism of associative inhibition. Kendler, himself, does not subscribe to this view but attempts to explain his findings in terms of the formation of anticipatory eating and drinking responses.

[15] This effect may become quite marked in learning situations which involve both a high motivational level and a need for flexibility of behavior. Hamilton's (1911) "persistent non-adjustive reaction" is a case in point.

motivation. In fact, primary reward is typically held to be nothing more than the satisfaction or reduction of some motivational state. If this is true, then it is apparent that without motivation there can be no reinforcement. Following this line of reasoning, it is easy to see that there may well be an interaction effect between amount of motivation and amount of incentive as regards the effect upon learning. If a particular incentive object is ineffective in the absence of motivation, but effective when the motivational state is acting, the question is, is this transition an all-or-none affair or does the reinforcing value of a particular incentive object change in a regular fashion as level of motivation is raised from zero to some high value? In another context, the same question may be asked in the following terms: if two groups practice for an equal number of trials with an equal level of reinforcement at the same task, and if these two groups differ only in level of motivation present during training, will a later test when the groups are also equated for level of motivation reveal a difference in the performance of the groups? Unfortunately, this experimental problem is an extremely complicated one to undertake to solve. The complicating factor is that the drive levels present during training result in different drive stimuli which, as we have seen, may become part of the stimulus complex which elicits the learned activity. Because of this, when the motivational levels are equated for the final test in the experiment mentioned above, the measurement includes the factor of stimulus generalization as well as the factor of differential learning under different drive states. Because of this complication a solution to this problem awaits the performance of a major piece of research employing an elaborate experimental design. At the present time, no unequivocal data exist with respect to this problem. It seems likely, though, that such drive-reward interaction effects do exist, although it would be hazardous to attempt to formulate the nature of this relationship at the present time.

Incidental Learning

The problem of incidental learning should be mentioned in this connection. We have seen that the factor of motivation is a potent one in determining learning and performance, but even when there *seems* to be no motive, people still learn. Learning under such conditions is called incidental learning and may be defined as *learning which apparently takes place without a specific motive or a specific formal instruction and set to learn the activity or material in question.* It is understood, of course, that the material must have stimulated a receptor and that no formal administration of reinforcement is involved. As we shall see, there is some doubt of whether learning occurs when motivation and reinforcement are lacking, but on casual inspection it would appear that there are numerous examples of this type of learning. When a subject who is under instruction to learn a list of words presented on a memory drum learns also that the hood of the drum has a crackle finish, or that the base is of varnished oak, or that the windows are made by sliding movable shutters along parallel grooves, he is doing incidental learning. So is he if he learns the contents of the experimental room or the clothes the experimenter wears. Much of the learning of everyday life is of this incidental sort. One drives along a road, walks down a street, meets friends and talks to them, and afterwards remembers much that seems to have been entirely separated from any specific motive which had been operating or any reinforcement which may have occurred.

Under such circumstances learning certainly does occur, but it also fails to occur and perhaps more frequently than not. In each of the situations mentioned above, there is much which comes within the sensory range of the individual but which he does not learn. Many of the familiar stimulating conditions of daily life leave no reportable impressions on us. The houses on often-traversed streets, the details of an often-entered room, whether or not an acquaintance wears glasses, and many another such item may never be learned. One difficulty with the study of incidental learning by such anecdotal methods is that we have no estimate of the ratio

of learned items to potentially learnable items. A second difficulty involves the problem of excluding from the learning situation all forms of motivation and reinforcement which might influence the acquisition process. Experimentally, incidental learning is usually studied by exposing subjects to stimulation under conditions which seem to exclude motivation to learn and then by testing to see if learning has occurred. It is not easy, however, to make sure that the subject actually observes the material presented, and at the same time has no intent to learn it. If he does not perceive the material, a test for learning is meaningless. If he is in any way set to learn, the result comes under the heading of learning as a function of the influence of set. Because of the difficulty attending the control of set, and because of the uncertainty that learning occurs in the absence of set (or other motivation), the word "apparently" has been used in the definition given above. Many of the experiments published under the name of incidental learning have so obviously left room for the arousal of a set to learn that that their results need not be considered here.

Subjects were asked by Myers (1913) to count rapidly the O's distributed among other letters printed in color on colored paper. They were then asked what letters there were besides O's, how many lines of letters there were, what was the color of the paper and the letters, and similar questions. The frequency of correct answers varied with the item, but was quite small. Few other letters were recalled; colors were fairly well recalled but were often ascribed to incorrect parts of the material; and the border was either not recalled at all or was recalled incorrectly.

Shellow (1923) has employed a variety of devices for insuring perception while excluding operationally induced sets to learn. In one experiment, the subjects were instructed to learn the names of magazines by looking at their covers and were asked later not only the names, but also a number of questions about the pictures and other features of the covers. In another, the subjects were asked to estimate the areas of differently colored forms and later were asked for recall and recognition of certain aspects of these materials. These and other methods gave clear evidence of incidental learning

in spite of the fact that care was exercised to avoid arousing an explicit intent to learn.

Biel and Force (1943) have used a similar device for assuring perception, presumably without arousing intent to learn. In this case, subjects were told that the experiment was concerned with the legibility of different typefaces, but were later asked to recall the materials printed in these different forms. Their results, like those obtained by Shellow, indicated that learning occurs under these circumstances. Their data further indicate that, when level of learning is equated, incidentally learned material is retained as well as intentionally learned material.

Work carefully done by the *Aussage* method also yields evidence for incidental learning. To subjects who are not instructed either to observe or to learn, a picture or an event is presented with a later request either to write a description of it or to answer questions about it. This will be recognized as being analogous to everyday situations on which people are sometimes asked to give testimony in court and which they frequently recount to others. These experiments yield substantial evidence of learning, but the material presented is far from being completely recalled and that which is recalled is by no means free from error. Furthermore, it is entirely possible that such situations as the ones usually employed in these experiments are not entirely free from hidden sources of secondary reinforcement.

Analysis of the experimental data shows that while subjects are practicing a given act or material with intent to learn it they may also learn other things toward which they are not directly motivated by the formal conditions of the experiment, and that when stimulated by material without instruction or formally controlled motivation to learn, they still learn. The question remains whether sets uncontrolled by the experimenter have been operating. A valuable answer to this question has been given in one important case by Jenkins (1933). In this case, a student who thought he was serving only as experimenter, read each syllable of a list of 20 as it appeared in the window of a memory drum to a subject who could not see the window and who was instructed to learn the syllables as they were

read to him. The experimenter was told to read each syllable clearly and distinctly, inasmuch as the experiment depended upon the accuracy of his reading. When the memorizer, on appropriate test, had made one successful recitation, both he and the experimenter were asked to return after 24 hours "to complete the learning." On their return, each was asked to write a recall and to report on experiences during learning and reproducing. Data were obtained for 24 who had served as subjects and for 24 who had served as experimenters unaware that a recall would be expected of them.[16] The mean recall under the instruction to learn was 15.9 ± 2.4, and under the instruction only to read clearly was 10.8 ± 3.6 (the measures of variability are average deviations). This is in agreement with the results of other experiments on incidental learning, but much more important for the present question are the reports of the subjects concerning their activities while serving as readers. Ten of the 24 "experimenters" reported deliberate and self-instructed attempts to learn, and only 8 asserted definitely that they did not try to learn.[17] The reports demonstrate, however, that the absence of a definite attempt to learn does not mean the absence of sets of the kind which influence learning. Indicated in the reports are fleeting and casual sets established by the materials and conditions. One syllable was remembered because the subject persistently missed it. Another was remembered because it was the last in the list, and so on. Jenkins found no subject who failed to report the presence of occasional instruction, and such factors as these, together with the meanings suggested by the syllables, will account for a great deal of the learning that occurred. These results show a definite set to learn on the part of some of those who were reading the syllables without formal instruction to do anything but read, and the presence of self-instructions or casual instructions in all. The inference is so plausible as to be almost inescapable that the learning

[16] Alper (1946) has used a similar method in order to study incidental learning under conditions of ego-involvement. Unfortunately, her data may be interpreted in terms of rehearsal (1948).

[17] These eight subjects recall 8.0 ± 2.0 syllables after reading. The absence of reported set to learn is accompanied by a smaller recall than is the presence of reported set, but the recall is still considerable.

which occurred without formal instruction to learn was, nevertheless, learning under the influence of instructions and sets of other kinds. Certainly, so long as such sets were present, we cannot conclude that any of the learning was uninfluenced by instruction and set. It is probable that all was influenced by it.

In this connection, the experimental findings and theoretical analysis of Postman and Senders (1946) may be cited. Their results indicate that, although their subjects were instructed to learn particular aspects of the material presented to them, they nevertheless learned other aspects of the material as well. Postman and Senders point out that the subjects may have "overt sets" to memorize the material, either as a result of formal instructions given by the experimenter or as a result of explicit self-instruction. They also conclude that "covert sets," not necessarily reportable by the subjects, may also operate to produce learning. This latter factor is of undoubted importance in incidental learning experiments. For example, in cases where the subject is instructed to read, but not instructed to memorize, certain material, it is nevertheless probable that the subject has an implicit set toward memorization. This is because reading habits have been established which point toward the comprehension and retention of the material read. These habits are so thoroughly established that we may consider the mere act of reading to be a learning activity and that we may question whether it is possible for an individual to read material in a completely passive manner and without acquiring some knowledge of the content of the material read.

In harmony with this conclusion are the reports of Shellow's (1923) subjects who describe the use of mnemonic devices of exactly the same sort as those employed in intentional learning. The vividness or the affective tone of colors, the peculiarities of personal experience, and the positions of the items, for example, were used as aids. The reports imply the arousal of a set to learn during the observation of the material. Shellow concludes that intentional and incidental learning are alike "in that fixation depends on some response" of the subject. Though learning may occur without apparent motivation, actually the subject is set or directed

toward parts of the material by reason of habitual modes of attack, interests, meaning, or by varied self-instructions. When we couple with these considerations the complex and subtle influences exercised by the behavior repertoire of the individual upon the fixation of new responses, together with the possible influence of spread of effect (Chap. VII), each of which is a function of some motivating condition, the stage is set for an acceptance of the statement that incidental learning is only "apparently" unmotivated.

SPECIAL METHODS OF CONTROLLING HUMAN MOTIVATION

Certain methods have been used in experiments on human learning to vary the motivational state of the subject. A brief review of the more important of these is offered here together with a few examples of characteristic experimental results obtained in each situation.

The Influence of Addition and Removal of Special Incentives

An attempt to control the individual's motivational level by use of special incentives represents one of the most widely used techniques in the practical control of human behavior. Actually, of course, this method is really concerned, at least in part, with the study of learning as a function of amount of reward. Additional information on this subject, therefore, is to be found in Chapter VII.

An experiment by Book and Norvell (1932) illustrates the general influence of the introduction of a group of special incentives and of their later shift from one group to another. A control group of university upper-class students were given seventy-five practice periods of thirty seconds each at making the small letter "a" as accurately and as rapidly as possible without the addition of any special incentive other than the instruction to work as rapidly and as conscientiously as they could. An experimental group worked under similar conditions, plus the following incentive conditions: (a) knowledge of score after each trial; (b) frequent encouragement between trials; (c) urging to do their best; (d) instruction to be on the lookout for any method which would aid in the learning; and

(e) the occasional statement that they should be doing better. At the beginning of the last third of practice each subject in the control group was given a record of the best score made in any preceding period and during the rest of practice received the incentive conditions listed above. These incentives were withdrawn from the other group (previously labeled "experimental") during the same period.

These experimental conditions are not analytic or clean-cut. A number of different incentive conditions are applied and no isolation of the individual influence of each is possible. Furthermore, some of the incentives appear to be more in the nature of guidance and instruction than to be motivational in character. It is possible, then, only to say that some effect occurs and that a change in score follows the removal of all of the incentive conditions. The experiment is cited both as an illustration of the general technique and findings of such experiments and because the results appear to be roughly analogous to some of those obtained under similar conditions with infrahuman subjects.

The curves in Figure 26, which are for a total of 48 men, are an example of the results. The practice periods are plotted in groups of five, so that there are only 15 divisions of the abscissa. The curves of the two groups are close together during the first 25 trials, but diverge during the next 25 until, at the 50th trial, the experimental group is considerably superior to the control. The shifting of the incentive conditions from one group to the other is followed by a reversal of the levels of performance. It will be noted that, although the learning curve for the control group does not rise so fast as that for the experimental group, it still rises, presumably as a function of the motive-incentive conditions aroused by the instructions and the entire experimental situation. To these motive-incentive conditions, which may be assumed to have an equal average strength in both groups, the special incentives were added.

A more analytic investigation by Abel (1936), which employed but one incentive at a time, likewise demonstrates that the addition of a special incentive accelerates learning. It shows, further, that the introduction of an effective incentive (a penny) after each trial

during the last half of practice in a multiple-U maze is equal in effect on nine-year-old boys to the same incentive after every trial.

Praise and reproof are incentives which are much used as pedagogical devices. Unfortunately, these are difficult to isolate as single variables, and in many of the experiments social approval and rivalry are added to praise and social disapproval to reproof. In an experiment by Hurlock (1925), four equated groups of grade-school children practiced addition under four different incentive conditions. The control subjects worked in a separate group and without

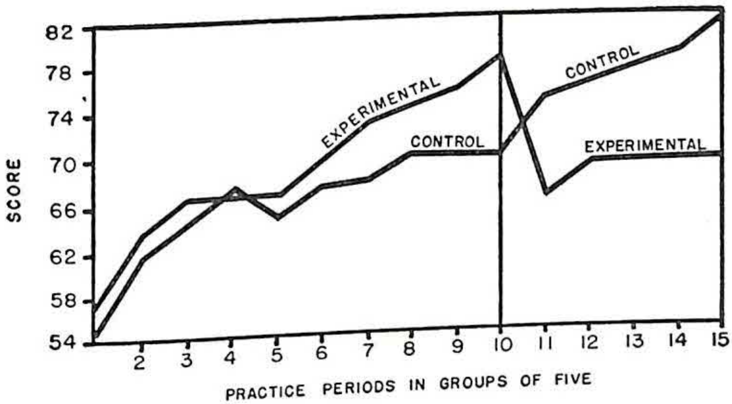

Fig. 26. Curves Showing the Influence of Special Incentives and of Their Removal

(From Book and Norvell, *Ped. Sem.*, 1922, 29, p. 323)

comment. The three other groups worked together but under different reward-incentive conditions. The names of those in the *praised* group were read and the children were asked to come to the front of the room and face the class where they were praised and encouraged. The members of the *reproved* group were called out and severely reproved for careless and inferior work, while those in the *ignored* group heard the praise and reproof given to the others but received no notice themselves. The mean scores (Table X), which are equal on the first day become different thereafter. The controls show no consistent gain from practice, the ignored group gains slightly at first but later falls below the level of its second day, while the reproved subjects show a more substantial gain but do not

maintain it, and the praised group a larger and more consistent gain. Reproof, which is a poor second to praise at the last practice period, is equal to praise on the first and second days of their application but becomes inferior thereafter. These special incentives give the results mentioned in spite of the fact that the subjects must have been already fairly well practiced in the activity of adding. Other experiments, likewise, have found praise and encouragement more effective than reproof, although reproof may still have a positive effect (Gilchrist, 1916; Abel, 1936).

TABLE X

SCORES IN ADDITION UNDER FOUR DIFFERENT INCENTIVE CONDITIONS

(From Hurlock, *J. educ. Psychol.*, 1925, 16, p. 149)

Group	Practice Day 1	2	3	4	5	Day 5 P.E. Av.
Control	11.81	12.34	11.65	10.50	11.35	.55
Praised	11.81	16.59	18.85	18.81	20.22	.99
Reproved	11.85	16.59	14.30	13.26	14.19	.87
Ignored	11.84	14.19	13.30	12.92	12.38	.79

Another negative form of incentive is electric shock. When electric shock is employed as an incentive during practice, it is commonly administered for particular responses, such as entering a blind alley, and not at the end of a trial or in some other way which does not refer to specific behavior. There is a very large body of data regarding the administration of shock for erroneous responses, the general trend of results indicating that the use of shock in this way facilitates the learning process. Following the pioneer study of Bunch (1928) which showed a definite acceleration of learning (50 per cent reduction in trials, 30 per cent reduction in time and errors) when shock was administered for erroneous responses, numerous other studies have confirmed this effect (Bunch and McTeer, 1932; Crafts and Gilbert, 1934; and others). The facilitation of learning appears also in mirror-drawing where shock can be administered when the tracing stylus touches the boundaries of the path. Barlow's (1933) subjects, for example, received a shock on

the wrist when the stylus touched the notched walls on the inside of the right half of a six-pointed star path or the walls of the outside of the left half, with the results shown in Table XI. The experimental group, which received shock for errors, took more time during the twenty circuits than did the controls and made fewer errors on both the shock side and the non-shock side. The increase in time does not necessarily contradict Bunch's result with a maze, because his subjects learned to a criterion while Barlow's stopped after twenty circuits of the star, and the reduction of total trials which Bunch found carried with it a reduction in total time necessary to reach the criterion. The two experiments agree in finding that shock increases time per trial. The decreased appearance of the unshocked errors implies a general motivational influence from the shock in the other sections of the pattern. The decrease in errors and increase in time per trial have been verified by McTeer (1933) who administered shock for errors on all sides of the star.

TABLE XI

MEAN TIME AND ERROR RECORDS IN MAKING 20 CIRCUITS OF A SIX-POINTED STAR

(From Barlow, *Amer. J. Psychol.*, 1933, 45, p. 480)

Group	N	Time Mean	Errors Shock Side	Errors Nonshock Side
Experimental	23	1202.8	175.9	238.2
Control	30	990.2	286.3	302.2

The influence of electric shock upon learning may be demonstrated in a number of other learning situations (McTeer, 1931; Bunch and Hagman, 1937; and Gurnee, 1938), when administered non-informatively (Bunch, 1935; Gilbert, 1936, 1937; Bernard, 1941, 1942; and Bernard and Gilbert, 1941), and even when administered for correct responses (Tolman, Hall and Bretnall, 1932; Muenzinger, 1934a, b).

Electric shock differs from other operationally controlled incentives in that it is usually administered for particular responses (for

example, in the blind alley rather than at the end of the maze pattern), and in that it normally elicits withdrawing behavior. The conditions and modes of its action may be discussed, therefore, separately from those of other incentives.

(a) *Intensity* of shock is a variable which may be measured accurately on the electrical side and roughly on the side of the subject's judgment. The available data are unanimous in showing that the intensity of shock most favorable to learning lies somewhere between the extremes of a subjective rating scale of intensities. "Heavy" shock may inhibit learning rather than facilitate it. The influence of any given intensity is a function of the units in which learning is measured, of the point in practice at which it is given, and doubtless of other conditions (Bunch, 1928; Vaughn and Diserens, 1930; McTeer, 1933).

(b) The *number of trials* during which it is given and the *stage of practice* during which these trials occur have been systematically studied by Bunch (1935) in an experiment which clearly demonstrates the accelerating influence of shock for errors in maze learning. Shock was administered during 2, 4, 8, or 12 initial trials and also after the subjects had had 2 or 4 trials before the introduction of the shock. Shock for a limited number of trials is not greatly different in effectiveness from shock given throughout practice. Whether the trials with shock are initial ones or occur after 2 or 4 trials without shock, the influence of the shock is nearly at a maximum when given for 2 or 4 trials and increases very slightly when given for 8 or 12 trials. With respect to stage of practice, the influence of the trials without shock is somewhat greater when introduced after 4 trials without shock. The few other data on this problem corroborate these in implying that shock during part of practice is nearly as effective as during all, and that shock during a part of practice is more effective if it is introduced after a few initial trials (Valentine, 1930; Travis, 1938).

(c) The influence of shock varies with the *specificity of its relation to a particular act*. Shock for specific acts is more effective in the elimination of individual errors than is non-specific shock,

although shock which is not specific to individual acts has a general accelerating influence. This general effect is implied by the fact that shock during as few as 2 or 4 trials accelerates learning nearly as much as when it is given for 8 or 12 trials (Bunch, 1935), and has been independently established by the work of Gilbert (1936, 1937), Bernard (1941, 1942) and Bernard and Gilbert (1941).

(d) The influence of shock seems also to be a function of the *activity practiced*. Shock administered when the stylus loses contact with the target in a form of pursuitmeter is only slightly facilitative, if at all (Travis, 1938; Travis and Anderson, 1938). It is readily conceivable that shock should disrupt a delicate, difficult, and continuous eye-hand coordination of this kind. Shock given during the last two seconds of a four-second exposure of irregular designs yields a decrease in the number of correct recognitions (Gurnee, 1939), possibly because the shock is not regarded as an integral part of the practice situation. It is to be expected that there are many activities which shock will disrupt by eliciting other and interfering responses. This disruptive aspect as a function of the activity practiced is similar in principle to the inhibiting action of high intensities of shock.

(e) *The other motive-incentive conditions effective at the same time* may frequently be sufficient to produce relatively rapid learning, and the addition of a given degree of shock may add little. Muenzinger (1934b) has suggested that with adult subjects the experimental instructions may yield a rate of learning so high that the addition of special incentives has little influence. This possibility is also implicit in Hull's theoretical formulations (1943), and has received empirical support at the infrahuman level by the data of Amsel (1950). This condition may account for some of the instances in which shock has seemed to have no influence on the learning process.[18]

[18] There are a number of variables which are worth considering but which need only to be listed, such as the part of the body to which the shock is administered, the unit in which learning is measured, and the individual characteristics of the subjects. For a discussion of the mechanisms by means of which punishment may influence learning, see Chapter VII.

The Influence of Rivalry, Success and Failure, and Related Conditions upon the Learning Process

Under the usual experimental conditions, subjects practice without specific formal information about either their own records or those of other subjects. By instruction and information, however, the experimenter may attempt to make his subjects compete either against their own previous records or against the scores of others. The control of such incentives cannot be rigid, but the influence of these conditions, even though unanalyzed and not rigidly controlled, is still important to know.

The results shown in Table XII represent a comparison between learning when the members of two subgroups compete with each

TABLE XII

INITIAL SCORES AND GAINS IN SUBSTITUTION AND READING UNDER THREE DIFFERENT INCENTIVE CONDITIONS

(From Sims, *J. educ. Psychol.*, 1928, 19, pp. 481 and 483)

Group	Substitution Initial Score	Substitution Per Cent Gain	Reading Initial Score	Reading Per Cent Gain
Control	36.0	102.2	167.3	8.7
Experimental 1 (Group rivalry)	36.1	109.9	167.5	14.5
Experimental 2 (Indiv. rivalry)	36.2	157.7	167.7	34.7

other (Experimental group 1) and when matched pairs of subjects compete with each other (Experimental group 2). In both cases the scores of competitors were known. All groups have large gains in substitution, but the competition between matched pairs yields the largest gain. Group rivalry is only a little more effective than the control condition. The gains in rate of reading are smaller, but the direction of the differences is the same. Either form of rivalry is superior to the control, but individual competition is superior to group competition (cf. Maller, 1929).

In a number of experiments, stimuli to rivalry have been found to yield faster learning than do the control conditions (Hurlock, 1927; Zubin, 1932). One should not conclude, however, that such

stimuli always and uniformly accelerate learning. They could almost certainly be employed in amounts and along dimensions which would not accelerate learning or which might actually retard it.[19]

When the experimenter gives controlled information regarding the relative status of the learner with respect to his competitors (either by falsifying the learner's score or the competitor's score, or both), we have an experimental situation in which success and failure can be controlled. Using such a method Sears (1937) demonstrated that experienced failure caused both an impairment of the efficiency with which the failed task was performed and also caused a transfer of impairment to another task which immediately followed. Similar results, as concern immediate performance, have been obtained by Russell and Farber (1948). They found that experimentally produced failure resulted in a decrement of performance on immediate tests of retention. When, however, retention was tested a week following learning a reversal of these results was obtained. That is, the failure group showed superiority to a success group which had exhibited superiority initially. It is possible that these results are continuous with those which seem to indicate that retention of material learned under "ego-orientation" is better than the retention of material learned under "task-orientation" (cf. Shaw, 1944; Shaw and Spooner, 1945; and Alper, 1946, 1948).[20]

The Effects of Instruction and Set

The early papers on learning contain frequent observations upon the importance of "attention," "the will to learn," and other concepts belonging to the class of *set* and *motive*. Although these observa-

[19] An experiment by McKinney (1933) is a case in point. His conditions are not labeled "rivalry," but they might be construed as belonging in that class. Their influence was in the direction of retardation of learning.

[20] The relationship between rivalry, success and failure, level of aspiration, ego-involvement, and praise and reproof, is a close one. It is probable that the common mechanisms of excitement, frustration, and anxiety underlie this relationship. The reader would do well to consult the reviews of the work on level of aspiration by Rotter (1942) and by Lewin, Dembo, Festinger, and Sears (1944) and the works of Allport (1943), and Sherif and Cantril (1947) on the relation of ego-involvement to learning.

tions were casually made and anecdotal, they led to the same conclusion to which experimental work has later brought us. A case in point is the incident described by Radossawljevitch (1907) of the subject who, because of his imperfect German, failed to comprehend the instructions in an experiment on verbal learning. The subject sat before a memory drum and read aloud a series of eight syllables time after time, but at the forty-sixth repetition he had not yet signaled that he had mastered the list. At that point, Radossawljevitch stopped the apparatus and asked if he could recite the series. "What! Am I to learn the syllables by heart?" was the reply. He could not recite them and required six more repetitions before he could. He had been observing and repeating the syllables, but, lacking an understanding of the instructions to learn, he had not learned them. The repetitions had probably had some effect, but not nearly so much as would be expected from 46 trials with a set to learn.

A similar instance is described by Sanford (1917), who had read the Order for Daily Morning Prayer provided by the Episcopal Church at least 5000 times in 25 years, usually at 24-hour intervals and often for many weeks in succession, yet was unable to recall the prayers unaided. At a recall test, 44 promptings were needed for the recall of a prayer of 124 words, and 27 for the recall of another having 158 words. Analogous cases will occur to anyone. An instructor who calls his class roll may read it three times a week for a semester, yet, despite this amount and distribution of practice, may be unable to recite three consecutive names without the book. One may observe and repeat the license number of one's automobile many times in a year, yet never learn it, and so on.

The conclusion to which these observations lead—that instruction and set to learn are much more effective for learning than is the set merely to observe—is amply supported by experimental evidence. Poppelreuter (1912) reports the learning of a list of 12 syllables in 12 readings with the instruction to learn, while with an attitude merely of attentive observation more than 50 readings were required. A large difference between the influences of the two sets also appeared when the method of paired-associates was used.

When subjects sort materials according to certain designated characteristics, learning is greater when the sorting is with intent to memorize the materials than without it (Mulhall, 1915). Forms and colors observed under instruction to assume a passive attitude are learned less well than when observed under instruction to learn (Wohlgemuth, 1915). Maso (1929), for example, gave subjects 12 pictures of objects and 12 cards carrying the names of the objects but in a mixed-up order with reference to the pictures, and instructed them to match the pictures and the names. The control subjects were given the already matched pictures and names and were told to examine them attentively. In this case, as in other similar experiments, active classification of, or search for, the materials used favored learning more than passive observation or study without active search.

It was observed early in the work on learning that specific instructions and sets exercised a very specific influence on the subject's learning. Meumann (1912) has made much of this. If a subject is told to learn a list in trochaic rhythm and assumes that the learning will be tested by the method of right associates, he sets himself to learn the unaccented syllables. When tested for learning of the accented syllables, he may show very little evidence of having learned them.

Similarly, Woodworth (1915) read a list of twenty pairs of unrelated words to adults, instructing them to learn so that the second member could be recalled when the first was given as a stimulus. After three repetitions of the list, the subjects were tested in this way, but they were also tested by being given the second word of a pair and asked to give the first word of the following pair. The second members, which they were instructed to learn, were recalled in 74 per cent of the cases, but the first member of the next pair, which they had not been specifically instructed to learn, gave only a corresponding 7 per cent.[21] The subjects reported, as one would expect, that they had tried to connect the members of each

[21] This work is operationally similar to the work which Thorndike (1931, 1932) has conducted on the factor of belonging (see Chapter VIII), and it is apparent that belonging may exert its influence mainly because the form of the material arouses a set to learn the material in a particular way.

pair and had neglected the sequence of the pairs as being unimportant. The first member of the successive pairs had been presented after the second member of the preceding pairs as often as the two members of a single pair had been presented together, but this frequency without a set to learn had brought very little learning. This is a fact which is of importance for the problems of incidental learning and of frequency as a condition of learning.

Formation of conditioned responses has been shown to be delicately and frequently susceptible to the influence of set. Conditioning of human salivary responses which, without control of set, may be irregular and variable becomes much more regular and constant when the subjects are under instruction to connect the two stimuli or when they are occupied during conditioning by the performance of a manual task. Instructions of other sorts have also been shown to be effective (Razran, 1935, 1936). Further, when subjects are instructed to assume an expectant attitude and await the stimulus, formation of conditioned eyelid responses is more frequent and more stable than when the instruction is to adopt a passive attitude (Grant, 1939). In many such ways, instructions influence the rate and other characteristics of conditioning, and the variable of instruction and set becomes an important one in the conditioning of human subjects.[22] The implications of the results in this field parallel and corroborate those of the classical learning experiments.

The set of the subject not only directs him toward an attack upon a rational problem; it also determines to a considerable extent what prior knowledge he shall bring to bear on it (transfer of training) and how he shall attack it. The classical data on set as a condition of what is recalled in association experiments, where the set to give a word of a particular class (an opposite, for example) acts as a selective device, are well known. Almost as well known is the fact of the directional character of thinking (Pratt, 1928; Gibson and McGarvey, 1937). The course of thought is a function of sets

[22] A bibliographical introduction to the work on set and conditioning will be found in the papers by Razran (1935, 1936), Mowrer (1938), and Grant (1939). Hilgard and Marquis (1940) give both bibliography and a discussion of the experimental results.

which arise from instructions, whether formal ones, the less formal ones stimulated by the material itself, or those given by the subject to himself.

The influence of a set engendered by the problem situation is illustrated by the work of Woodworth and Sells (1935) and Sells (1936) on the "atmosphere effect," by which is meant the set to complete a task with the one of several solutions which is most in line with the general trend of the problem. An example would be the case where, in examining a syllogism, a negative premise arouses a negative set or atmosphere. The results demonstrate that this "atmosphere effect" is a powerful determiner of the subject's conclusion and that the brighter subjects are more susceptible to it than are the duller ones.

The influence of set in practically all kinds of problem solution must be very great. It has been shown, for example, that if a number of problems are successively solved by the same method, solution of a new problem involving a different method becomes more difficult in the sense that it is less likely to occur (Luchins, 1942).[23] In perceptual-motor problems such as the maze, it may direct the subject's mode of attack; in puzzle-solution it may be a determiner of his attempted solutions and of their order of trial; in *Umweg* problems it may direct him toward or away from orientation toward the detour which will solve the problem; and in all sorts of relational problems it may lead him to look for and adopt certain relations as the crucial ones whether they are or not. In all of these cases it is probable that set acts to a great extent by selecting what prior training shall transfer to the present problem.

The data which have been cited, and many others, point to the conclusion that an active set to learn, with its accompanying selective process and active response to the material practiced, is a

[23] The effect of set upon problem solution involves the clinical concept of rigidity, a thorough discussion of which would take us too far afield here. It is believed that this type of rigidity is greater with conditions of mental deficiency (Lewin, 1935; Werner, 1946), schizophrenia (Kasanin and Hanfman, 1938), brain injury (Goldstein, 1939, 1943; Straus and Werner, 1943) and certain other personality characteristics (Frenkel-Brunswik, Levinson, and Sanford, 1947; Rokeach, 1948). The reader is also referred to articles by Cattell (1935, 1946) and by Werner (1946).

powerful determiner of learning, whether the learning be the fixation of a verbal series, the establishment of a conditioned response, or the discovery and fixation of the solutions of perceptual-motor and rational problems. The set may be established by formal instructions or may arise from the experimental situation and the subject's own reaction systems.

Experimentally Induced Muscular Tension

The first systematic study of this problem was published by Bills (1927). His subjects learned lists of nine nonsense syllables by the anticipation method, at the same time maintaining continuously with each hand a fairly comfortable degree of pressure on

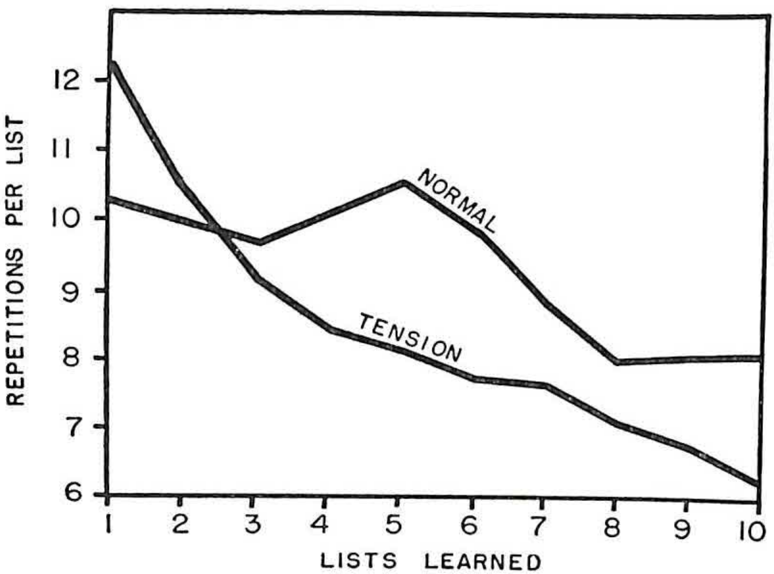

FIG. 27. PRACTICE CURVES UNDER NORMAL AND INDUCED TENSION (From Bills, *Amer. J. Psychol.*, 1927, 38, p. 235)

the handles of a spring dynamometer. Under a control condition, the same subjects learned similar lists but without pressure on the dynamometers. Mean repetitions per list, plotted against the serial

order of the list in practice (Fig. 27), show the condition of induced tension to be superior after the second list. That the facilitation from tension is not gained at the expense of other customary measures of learning is shown by the fact that tension is also accompanied by a larger overlearning score and by a higher recall and savings score after three hours. The facilitative influence of induced tension was demonstrated also in a second experiment with paired associate learning. The amount of the difference between the tension and the control conditions varies from subject to subject and from material to material, but the trend is toward faster learning under induced tension. This trend has been corroborated by other experiments, to some of which we shall refer when discussing the conditions of which the influence of tension is a function.

Stroud (1931) has shown that the facilitative effect of tension appears also in maze learning. His subjects learned two mazes in a counterbalanced order, one in the usual manner and the other while holding a weight with a static pull of fourteen pounds against a pulley. The subject sat upright and exerted the pull at right angles to his body, thus inducing tenseness in a large number of skeletal muscles. Simultaneously, he traced the maze with a pressure stylus having a piston device for recording downward pressure. The more difficult of the two mazes was learned more readily under tension, as was also the easier when the scoring was in terms of time and errors.

The effects of muscular tension may also be studied by assessing the amount of muscular tension which is spontaneously induced during learning, that is, when no special tension-inducing conditions are introduced. The relation of different degrees of such "normal" tension to rate of learning could be studied in Stroud's control condition, where the subjects traced the maze with a pressure stylus, but without induced tension. When the learning records of a group showing the greatest degrees of tension are compared with those of the group showing the lowest degrees (Table XIII), similar results are obtained. Tension, whether induced experimentally or not, is positively correlated with rate of learning. This is true, also, in mirror tracing where, parallel with electric shock for errors

(which increased rate of learning) went an increased tension in the inactive hand (McTeer, 1933).

TABLE XIII

DATA SHOWING RELATIONS BETWEEN TENSION AND RATE OF MAZE LEARNING

(Modified from Stroud, *J. exp. Psychol.*, 1931, 14, p. 622)

Maze	Tension Group	Time (Sec.)	Errors	Trials
Harder	Upper half	222	8.3	8.5
Harder	Lower half	485	33.0	12.1
Easier	Upper half	2426	321.0	40.9
Easier	Lower half	2916	527.0	48.9

The experimental results cited are characteristic of a number which have shown a positive relation between tension and rate of learning, but these results mean only that certain amounts of tension measured in certain muscles accompany faster learning under a given set of conditions. It would certainly be dangerous to conclude that increased tension always accompanies or produces faster learning under all conditions. Although the conditions determining the relations between these two classes of variables have not been thoroughly explored, there are a few of them about which enough is known to merit discussion.[24]

The effects of differing amounts of tension may vary widely under both normal and experimental conditions. On the basis of his own results and against the background of earlier work, Stauffacher (1937) formulated the hypothesis that there is an amount of tension which is optimal for the learning of a given activity, and that amounts of tension above and below this level are accompanied by slower rates of learning. This has been carefully tested by Courts (1939), using varying degrees of pressure on a dynamometer to induce tension, measuring the tension by the amplitude of the knee-jerk, and measuring the learning by the total number of

[24] For a thorough review of the literature to 1942, see Courts (1942a). Davis' (1942) article on the measurement of muscular tension is also recommended.

letters (in consonant syllables) correctly anticipated during the four recitation trials. The results (Fig. 28) support the hypothesis just stated and demonstrate that the relation between tension and rate of learning is a function of the amount of the former. Everyday observation is, moreover, continuous with this hypothesis. With general muscular relaxation, an individual is relatively ineffective, if, indeed, sleep does not supervene, while with the high degrees of tension present when one "tries too hard," for example, one is also ineffective. In the latter case, the supraoptimal degrees of tension may inhibit learning because they involve either a greater

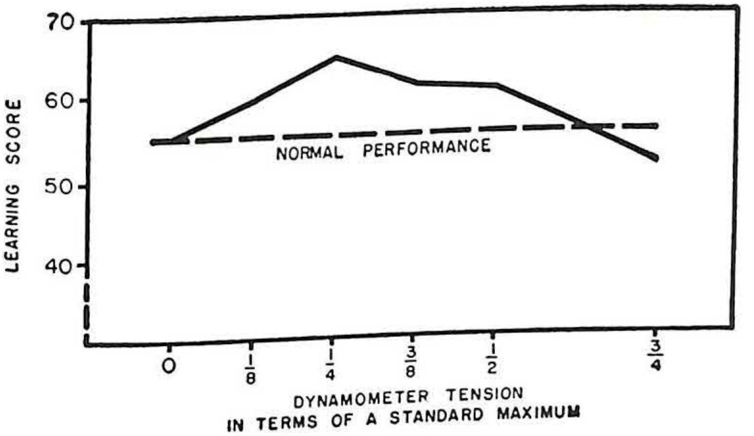

FIG. 28. MEAN LEARNING SCORES UNDER DIFFERENT DEGREES OF TENSION ON A DYNAMOMETER (N = 60)
(From Courts, *J. exp. Psychol.*, 1939, 25, p. 243)

amount of muscular contraction or a contraction of more muscles than can be directed satisfactorily into specific practice. In a later study, Courts (1942b) has demonstrated that the dynamogenic effect of induced tension is dependent, not only on amount of tension, but also upon level of performance.

A second condition is that of the muscle groups involved. Davis (1939) finds that for learning nonsense syllables there is a characteristic muscular activity which differs in spatial pattern and intensity from that present during multiplication and which, by implication, might be different from that obtained in other learning

tasks. In some cases, the increase in tension during motivated learning may be confined to a few muscles. The muscle groups involved, the distribution and amount of tension, may be variables in the frequent instruction to golfers, baseball players, and other athletes to "relax." Apparently, what is desired is such a distribution of tension that the muscles directly required by the skill are at optimal degrees of tension, while the remainder of the musculature is not sufficiently contracted to produce inhibitory responses.

Workers in this field have suggested that among the other conditions are the length of the practice period, the character and difficulty of the task, the measure of learning employed, and the interrelations of these variables among themselves and with the variables already cited.

Since experimentally induced tension is correlated with increased rate of learning, it is legitimate to regard learning as the dependent variable and to ask how it is influenced by tension. One reasonable interpretation is that tension operates by way of neural facilitation and inhibition. Increased muscular tension, however produced, must send to the central nervous system volleys of proprioceptive impulses. These impulses may converge upon the final common paths activated by the dominant stimuli and may summate to reinforce the responses to these stimuli; or these irradiating impulses may lower the thresholds of excitability and make conduction more ready in centers reached by impulses from the dominant stimuli. On the hypothesis of neural facilitation and inhibition it can be understood that tension in muscle groups remote from those directly concerned in the practice may facilitate learning (cf. Freeman, 1934). It likewise makes understandable the fact that tension great enough to produce high excitability and diffuse discharge to muscle groups involved in either related or competing responses should interfere with delicate coordination, or, if very great, with even the more gross coordinations. Therefore, it incorporates the fact that experimentally induced tension may facilitate at first and that supraoptimal amounts of tension inhibit. The hypothesis, as stated, is no more than an indication of one possibility.

Closely related to this hypothesis is Block's (1936) suggestion

that increased tension may facilitate learning by making the material more "vivid," an influence which might be exercised by way of a richer context of proprioceptive stimulation. Continuous with this is Guthrie's (1935) view that set consists of a group of maintaining stimuli, primarily proprioceptive, which dominate the individual's behavior. This view implies that set is muscular tension in readiness for response in a particular direction.

REFERENCES

Abel, L. B. The effects of shift in motivation upon the learning of a sensorimotor task. *Arch. Psychol., N. Y.*, 1936, 29, No. 205.

Allport, G. W. The ego in contemporary psychology. *Psychol. Rev.*, 1943, 50, 451-478.

Alper, T. G. Task-orientation vs. ego-orientation in learning and retention. *Amer. J. Psychol.*, 1946, 59, 236-248.

Alper, T. G. Task-orientation and ego-orientation as factors in reminiscence. *J. exp. Psychol.*, 1948, 38, 224-238.

Amsel, A. The combination of a primary appetitional need with primary and secondary emotionally derived needs. *J. exp. Psychol.*, 1950, 40, 1-14.

Barlow, M. C. The influence of electric shock in mirror tracing. *Amer. J. Psychol.*, 1933, 45, 478-487.

Bernard, J. A note on non-informative shock. *J. exp. Psychol.*, 1941, 29, 407-412.

Bernard, J. The specificity of the effect of shock on the acquisition and retention of motor and verbal habits. *J. exp. Psychol.*, 1942, 31, 69-78.

Bernard, J., and Gilbert, R. W. The specificity of the effect of shock for error in maze learning with human subjects. *J. exp. Psychol.*, 1941, 28, 178-186.

Biel, W. C., and Force, R. C. Retention of nonsense syllables in intentional and incidental learning. *J. exp. Psychol.*, 1943, 32, 52-63.

Bills, A. G. The influence of muscular tension on the efficiency of mental work. *Amer. J. Psychol.*, 1927, 38, 227-251.

Bills, A. G., and Stauffacher, J. C. The influence of voluntarily induced tension on rational problem solving. *J. Psychol.*, 1937, 4, 261-271.

Bird, C. *Social psychology.* New York: D. Appleton-Century, 1940.

Block, H. The influence of muscular exertion upon mental performance. *Arch. Psychol., N. Y.*, 1936, 29, No. 202.

Book, W. F., and Norvell, L. The will to learn: an experimental study of incentives in learning. *Ped. Sem.*, 1922, 29, 305-362.

Bunch, M. E. The effect of electric shock as punishment for errors in human maze-learning. *J. comp. Psychol.*, 1928, 8, 343-359.

Bunch, M. E. Certain effects of electric shock in learning a stylus maze. *J. comp. Psychol.*, 1935, 20, 211-242.

Bunch, M. E., and Hagman, E. P. The influence of electric shocks for errors in rational learning. *J. exp. Psychol.*, 1937, 21, 330-341.

Bunch, M. E., and McTeer, F. D. The influence of punishment during learning upon retroactive inhibition. *J. exp. Psychol.*, 1932, 15, 473-495.

Cannon, W. B. *Bodily changes in pain, hunger, fear and rage.* New York: Appleton-Century, 1915.

Carr, H. A. *Psychology: a study of mental activity.* New York: Longmans, Green, 1925.

Cattell, R. B. On the measurement of perseveration. *Brit. J. educ. Psychol.*, 1935, 5, 76-92.

Cattell, R. B. The riddle of perseveration. *J. Pers.*, 1946, 14, 229-238; 239-267.

Courts, F. A. Relations between experimentally induced muscular tension and memorization. *J. exp. Psychol.*, 1939, 25, 235-256.

Courts, F. A. The influence of practice on the dynamogenic effect of muscular tension. *J. exp. Psychol.*, 1942a, 30, 504-511.

Courts, F. A. Relations between muscular tension and performance. *Psychol. Bull.*, 1942b, 39, 347-367.

Crafts, L. W., and Gilbert, R. W. The effect of punishment during learning upon retention. *J. exp. Psychol.*, 1934, 17, 73-84.

Daniel, R. S. The distribution of muscular action potentials during maze learning. *J. exp. Psychol.*, 1939, 24, 621-629.

Davis, R. C. Patterns of muscular activity during "mental work" and their constancy. *J. exp. Psychol.*, 1939, 24, 451-465.

Davis, R. C. Methods of measuring muscular tension. *Psychol. Bull.*, 1942, 39, 329-346.

Diserens, C. M., and Vaughn, J. The experimental psychology of motivation. *Psychol. Bull.*, 1931, 28, 15-65.

Dollard, J., Doob, L. W., Miller, N. E., Mowrer, O. H., Sears, R. R., *et al. Frustration and aggression.* New Haven: Yale Univ. Press, 1939.

Dollard, J., and Miller, N. E. *Personality and psychotherapy.* New York: McGraw-Hill, 1950.

Farber, I. E. Response fixation under anxiety and non-anxiety conditions. *J. exp. Psychol.*, 1948, 38, 111-131.

Freeman, G. L. The facilitative and inhibitory effects of muscular tension upon performance. *Amer. J. Psychol.*, 1933, 45, 17-52.

Freeman, G. L. *Introduction to physiological psychology.* New York: Ronald Press, 1934.

Freeman, G. L. The optimal locus of "anticipatory tensions" in muscular work. *J. exp. Psychol.*, 1937, 21, 554-564.

Frenkel-Brunswik, E., Levinson, D. J., and Sanford, R. N. The anti-democratic personality. *In* Newcomb, T. M., and Hartley, E. (Eds.) *Readings in social psychology.* New York: Holt, 1947.

Ghiselli, E. Changes in neuro-muscular tension accompanying the performance of a learning problem involving constant choice time. *J. exp. Psychol.*, 1936, 19, 91-98.

Gibson, E. J., and McGarvey, H. R. Experimental studies of thought and reasoning. *Psychol. Bull.*, 1937, 34, 327-350.

Gibson, J. J. A critical review of the concept of set in contemporary experimental psychology. *Psychol. Bull.*, 1941, 38, 781-817.

Gilbert, R. W. The effect of non-informative shock upon maze learning and retention with human subjects. *J. exp. Psychol.*, 1936, 19, 456-466.

Gilbert, R. W. A further study of the effect of non-informative shock upon learning. *J. exp. Psychol.*, 1937, 20, 396-407.

Gilchrist, E. P. The extent to which praise and reproof affect a pupil's work. *Sch. and Soc.*, 1916, 4, 872-874.

Goldstein, K. *The organism.* New York: American Book Co., 1939.

Goldstein, K. Concerning rigidity. *Character and Pers.*, 1943, 11, 209-226.

Grant, D. A. The influence of attitude on the conditioned eyelid response. *J. exp. Psychol.*, 1939, 25, 333-346.
Gurnee, H. The effect of electric shock for right responses on maze learning in human subjects. *J. exp. Psychol.*, 1938, 22, 354-364.
Gurnee, H. The effect of mild annoyance upon the learning of visual forms. *J. exp. Psychol.*, 1939, 25, 212-220.
Guthrie, E. R. *The psychology of learning.* New York: Harper, 1935.
Guthrie, E. R. *The psychology of human conflict.* New York: Harper, 1938.
Hamilton, G. V. A study of trial and error reactions in mammals. *J. Anim. Behav.*, 1911, 1, 33-66.
Hilgard, E. R., and Marquis, D. G. *Conditioning and learning.* New York: D. Appleton-Century, 1940.
Hull, C. L. Knowledge and purpose as habit mechanisms. *Psychol. Rev.*, 1930, 37, 511-525.
Hull, C. L. Goal attraction and directing ideas conceived as habit phenomena. *Psychol. Rev.*, 1931, 38, 487-506.
Hull, C. L. Differential habituation to internal stimuli in the albino rat. *J. comp. Psychol.*, 1933, 16, 255-273.
Hull, C. L. Learning. II. The factor of the conditioned reflex. *In* Murchison, C. (Ed.), *A handbook of general experimental psychology.* Worcester, Mass.: Clark Univ. Press, 1934.
Hull, C. L. *Principles of behavior.* New York: D. Appleton-Century, 1943.
Hull, C. L. Behavior postulates and corollaries—1949. *Psychol. Rev.*, 1950, 57, 173-180.
Hurlock, E. B. An evaluation of certain incentives used in school work. *J. educ. Psychol.*, 1925, 16, 145-159.
Hurlock, E. B. The use of group rivalry as an incentive. *J. abnorm. soc. Psychol.*, 1927, 22, 278-290.
Jenkins, J. G. Instruction as a factor in "incidental" learning. *Amer. J. Psychol.*, 1933, 45, 471-477.
Kasanin, J., and Hanfman, R. An experimental study of concept formation in schizophrenia. *Amer. J. Psychiat.*, 1938, 95, 35-52.
Kendler, H. H. Drive interaction. I. Learning as a function of the simultaneous presence of hunger and thirst drives. *J. exp. Psychol.*, 1945, 35, 96-109.
Kendler, H. H. The influence of simultaneous hunger and thirst drives upon the learning of two opposed spatial responses of the white rat. *J. exp. Psychol.*, 1946, 36, 212-220.
Kendler, H. H. An experimental examination of the non-selective principle of association of drive stimuli. *Amer. J. Psychol.*, 1949a, 62, 382-391.
Kendler, H. H. An experimental examination of the selective principle of association of drive stimuli. *Amer. Psychologist*, 1949b, 4, 221-222. (Abstract.)
Leeper, R. The role of motivation in learning: a study of the phenomenon of differential motivational control of the utilization of habits. *J. genet. Psychol.*, 1935, 46, 3-40.
Lewin, K. *A dynamic theory of personality: selected papers.* (Trans. by D. K. Adams and K. E. Zener.) New York: McGraw-Hill, 1935.
Lewin, K., Dembo, T., Festinger, L., and Sears, P. S. Level of aspiration. *In* Hunt, J. McV. (Ed.), *Personality and the behavior disorders,* Vol. I. New York: Ronald Press, 1944.
Luchins, A. S. Mechanization in problem solving: the effect of Einstellung. *Psychol. Monogr.*, 1942, 54, No. 248.

Maller, J. B. Cooperation and competition. *Teach. Coll. Contr. Educ.*, 1929, No. 384.

Maso, N. La valeur de l'activité de l'esprit dans la fixation des idées. Contribution expérimentale à la théorie de l' "école active." *Arch. Psychol., Genève*, 1929, 21, 275-292.

McKinney, F. Certain emotional factors in learning and efficiency. *J. gen. Psychol.*, 1933, 9, 101-115.

McTeer, W. A study of certain features of punishment in serial learning. *J. exp. Psychol.*, 1931, 14, 453-476.

McTeer, W. Changes in grip tension following electric shock in mirror tracing. *J. exp. Psychol.*, 1933, 16, 735-742.

Melton, A. W. Learning. *In* Monroe, W. S. (Ed.), *Encyclopedia of educational research*. New York: Macmillan, 1941.

Meumann, E. Beobachtungen über differenzierte Einstellung bei Gedächtnisversuchen. *Z. pädag. Psychol.*, 1912, 13, 456-463.

Miller, N. E. Experimental studies of conflict. *In* Hunt, J. McV. (Ed.), *Personality and the behavior disorders*. Vol. I. New York: Ronald Press, 1944.

Miller, N. E. Studies of fear as an acquirable drive: I. Fear as motivation and fear-reduction as reinforcement in the learning of new responses. *J. exp. Psychol.*, 1948, 38, 89-101.

Miller, N. E. Learnable drives and rewards. *In* Stevens, S. S. (Ed.), *Handbook of experimental psychology*. New York: Wiley, 1951.

Miller, N. E., and Dollard, J. *Social learning and imitation*. New Haven: Yale Univ. Press, 1941.

Mowrer, O. H. Preparatory set (expectancy)—a determinant in motivation and learning. *Psychol. Rev.*, 1938, 45, 62-91.

Mowrer, O. H. A stimulus-response analysis of anxiety and its role as a reinforcing agent. *Psychol. Rev.*, 1939, 46, 553-565.

Mowrer, O. H., and Lamoreaux, R. R. Fear as an intervening variable in avoidance conditioning. *J. comp. Psychol.*, 1946, 39, 29-50.

Muenzinger, K. F. Motivation in learning: I. Electric shock for correct response in the visual discrimination habit. *J. comp. Psychol.*, 1934a, 17, 267-277.

Muenzinger, K. F. Motivation in learning: II. The function of electric shock for right and wrong responses in human subjects. *J. exp. Psychol.*, 1934b, 17, 439-448.

Mulhall, E. F. Experimental studies in recall and recognition. *Amer. J. Psychol.*, 1915, 26, 217-228.

Myers, G. C. A study in incidental memory. *Arch. Psychol., N. Y.*, 1913, 4, No. 26.

Pavlov, I. P. *Conditioned reflexes*. (Trans. by G. V. Anrep) London: Oxford Univ. Press, 1927.

Perin, C. T. Behavior potentiality as a joint function of the amount of training and the degree of hunger at the time of extinction. *J. exp. Psychol.*, 1942, 30, 93-113.

Poppelreuter, W. Nachweis der Unzweckmässigkeit die gebräuchlichen Assoziationsexperimente mit sinnlosen Silben nach dem Erlernungs—und Trefferverfahren zur exakten Gewinnung elementarer Reproduktionsgesetze zu verwenden. *Z. Psychol.*, 1912, 61, 1-24.

Postman, L., and Senders, V. L. Incidental learning and generality of set. *J. exp. Psychol.*, 1946, 36, 153-165.

Pratt, C. C. Experimental studies of thought and reasoning. *Psychol. Bull.*, 1928, 25, 550-561.

Radossawljevitch, P. R. *Das Behalten und Vergessen bei Kindern und Erwachsenen nach experimentellen Untersuchungen*. Leipzig: Nemnich, 1907.

Razran, G. H. S. Conditioned responses: an experimental study and a theoretical analysis. *Arch. Psychol., N. Y.*, 1935, 28, No. 191.

Razran, G. H. S. Attitudinal control of human conditioning. *J. Psychol.*, 1936, 2, 327-337.

Rokeach, M. Generalized mental rigidity as a factor in ethnocentrism. *J. abnorm. soc. Psychol.*, 1948, 43, 259-278.

Rotter, J. B. Level of aspiration as a method of studying personality. I. A critical review of methodology. *Psychol. Rev.*, 1942, 49, 463-474.

Russell, W. A., and Farber, I. E. Retention of verbal material as a function of degree of failure experienced in original learning. *Amer. Psychologist*, 1948, 3, 355. (Abstract.)

Sanford, E. C. A letter to Dr. Titchener. *Stud. Psychol.: Titchener Commem. Vol.*, 1917, 5-10.

Sears, R. R. Initiation of the repression sequence by experienced failure. *J. exp. Psychol.*, 1937, 20, 570-580.

Sells, S. B. The atmosphere effect: an experimental study of reasoning. *Arch. Psychol., N. Y.*, 1936, 29, No. 200.

Shaw, F. J. Two determinants of selective forgetting. *J. abnorm. soc. Psychol.*, 1944, 39, 434-445.

Shaw, F. J., and Spooner, A. Selective forgetting when the subject is not ego-involved. *J. exp. Psychol.*, 1945, 35, 242-247.

Shellow, S. M. Individual differences in incidental memory. *Arch. Psychol., N. Y.*, 1923, 10, No. 64.

Sherif, M., and Cantril, H. *The psychology of ego-involvements*. New York: Wiley, 1947.

Siegel, P. S. Alien drive, habit strength, and resistance to extinction. *J. comp. Psychol.*, 1946, 39, 307-317.

Sims, V. M. The relative influence of two types of motivation on improvement. *J. educ. Psychol.*, 1928, 19, 480-484.

Spence, K. W. The role of secondary reinforcement in delayed reward learning. *Psychol. Rev.*, 1947, 54, 1-8.

Stauffacher, J. C. The effect of induced muscular tension upon various phases of the learning process. *J. exp. Psychol.*, 1937, 21, 26-46.

Stone, C. P. Motivation: drives and incentives. *In* Moss, F. A. (Ed.), *Comparative psychology*, New York: Prentice-Hall, 1934.

Strauss, A. A., and Werner, H. Comparative psycho-pathology of the brain-injured child and the traumatic brain-injured adult. *Amer. J. Psychiat.*, 1943, 99, 835-838.

Stroud, J. B. The role of muscular tensions in stylus maze learning. *J. exp. Psychol.*, 1931, 14, 606-631.

Thorndike, E. L. *Human learning*. New York: Century, 1931.

Thorndike, E. L., *et al. The fundamentals of learning*. New York: Teachers College, Columbia Univ., 1932.

Tolman, E. C., Hall, C. S., and Bretnall, E. P. A disproof of the law of effect and a substitution of the laws of emphasis, motivation and disruption. *J. exp. Psychol.*, 1932, 15, 601-614.

Travis, R. C. Comparison of the influences of monetary reward and electric shocks on learning in eye-hand coördination. *J. exp. Psychol.*, 1938, 23, 423-427.

Travis, R. C., and Anderson, H. C. The effect of electric shock on learning in eye-hand coördination. *J. exp. Psychol.*, 1938, 23, 101-107.

Underwood, B. J. *Experimental psychology: an introduction.* New York: Appleton-Century-Crofts, 1949.

Valentine, R. The effects of punishment for errors on the maze learning of rats. *J. comp. Psychol.*, 1930, 10, 35-53.

Vaughn, J., and Diserens, C. M. The relative effects of various intensities of punishment on learning and efficiency. *J. comp. Psychol.*, 1930, 10, 55-66.

Watson, J. B., and Rayner, R. Conditioned emotional reactions. *J. exp. Psychol.*, 1920, 3, 1-14.

Webb, W. B. The motivational aspect of an irrelevant drive in the behavior of the white rat. *J. exp. Psychol.*, 1949, 39, 1-14.

Werner, H. Abnormal and subnormal rigidity. *J. Abnorm, soc. Psychol.*, 1946a, 41, 15-24.

Werner, H. The concept of rigidity: a critical evaluation. *Psychol. Rev.*, 1946b, 53, 43-52.

Williams, S. B. Resistance to extinction as a function of the number of reinforcements. *J. exp. Psychol.*, 1938, 23, 506-521.

Wohlgemuth, A. Simultaneous and successive association. *Brit. J. Psychol.*, 1915, 7, 434-452.

Wolfe, J. B. Effectiveness of token-rewards for chimpanzees. *Comp. Psychol. Monogr.*, 1936, 12, No. 60.

Woodworth, R. S. A revision of imageless thought. *Psychol. Rev.*, 1915, 22, 1-27.

Woodworth, R. S., and Sells, S. B. An atmosphere effect in formal syllogistic reasoning. *J. exp. Psychol.*, 1935, 18, 451-460.

Young, P. T. *Motivation of behavior.* New York: Wiley, 1936.

Zubin, J. Some effects of incentives: a study of individual differences in rivalry. *Teach. Coll. Contr. Educ.*, 1932, No. 532.

VII

THE LAW OF EFFECT

INTRODUCTION

THAT responses are learned as a function of their consequences is one of the most apparent facts of learning. Both in the realm of the practical control of behavior and in the laboratory investigations of human and animal learning, the administration of rewards (as well as punishments, knowledge of results, etc.) is a widely used technique. As an empirical fact, then, there is not a great deal of dispute concerning the law of effect. The use of effect as a theoretical principle is, however, another matter.

The *Empirical Law of Effect* [1] may be stated in different ways depending upon the aspects one wishes to emphasize. *Effect* means, throughout, what happens following the act in question, usually what happens within a very few seconds after it. The general relationship between fixation and effect is contained in *Statement I*, that *acts are fixated and eliminated as functions of their effects.* This is a statement that acts leading to certain kinds of effects are fixated or learned, while acts leading to other effects remain unchanged or are eliminated. Stated in this way, the law of effect suffers from overgenerality and from circularity in the sense that the effects which fixate and the effects which eliminate are unspecified except in terms of learning. If it is possible to specify the consequences which lead to fixation or elimination *independently of the learning situation,* the vagueness will be reduced and the implied

[1] The name "empirical law of effect" has been proposed by Carr (1938) for the observed facts that such conditions as food and shock determine what acts shall be fixated and eliminated. The term "empirical law" is particularly apt in view of the frequent confusion in discussions of effect between the facts to be explained and the proffered explanations. Statement I in the text is similar to Carr's earlier statement (1925) that fixation and elimination are explained in terms of their consequences.

circularity will vanish. A number of such specifications have been attempted, the best known, perhaps, being that of Thorndike (1931). This specification is made in *Statement II: Acts followed by a state of affairs which the individual does not avoid, and which he often tries to preserve or attain, are selected and fixated, while acts followed by states of affairs which the individual avoids or attempts to change are eliminated.*[2] Other things being equal, food (when one is hungry) and being told that one's response has been "right" are states of affairs which the normal individual does not avoid but seeks to attain, while experiencing electric shock and being told that he is "wrong" are states of affairs which he avoids or tries to change.

To go beyond this statement of the law of effect and to attempt to specify the conditions under which events are (or become) satisfiers and annoyers will be useful, particularly if we wish to specify a number of different mechanisms of effect. Such a statement will also contribute to an understanding of the general nature of effect. It will, however, depart from the purely empirical level, and, because of this, will unquestionably serve as a basis for theoretical dispute. A number of conditions of an event's being or becoming a satisfier may be listed.

1. An event may be considered to be a satisfier if it reduces the strength of some physiological or "primary" drive state of the organism. That is, stimulus-response relations which are followed by a reduction of primary drive strength tend to be learned. The experiments which have demonstrated this empirical fact are far too numerous to be listed here and, in fact, include the majority of animal learning experiments. It will be noted that circularity is avoided because drives and drive reduction and the organism's reaction to various primary-drive-reducing agents may be observed

[2] Thorndike uses this characterization as a definition of satisfiers and annoyers, respectively, terms which have often been misunderstood by some to mean subjective states, usually affective ones, but which are useful terms if understood as defined. This usage has much in common with that employed by Carr (1925, 1930). Other definitions would include those which are frankly hedonistic and those which are more biologically oriented. The primary requirement is that the nature of effect be independently defined.

independently. Thus animals, by and large, do not avoid food when they have been starved, nor water when they have been deprived therefrom. On the contrary, they actively approach these incentives and resist being removed from them.

Hull's (1943) statement of the *law of primary reinforcement* represents a precise statement of this principle.

> "Whenever an effector activity occurs in temporal contiguity with the afferent impulse, or the perseverative trace of such an impulse, resulting from the impact of a stimulus energy upon a receptor, and this conjunction is closely associated in time with the diminution in the receptor discharge characteristic of a need, there will result an increment to the tendency for that stimulus on subsequent occasions to evoke that reaction."

It will be noted that Hull identifies drive reduction with a diminution in drive-stimulation.[3] This is, of course, only one way of considering this matter, but it is probably the most precise method at our present state of knowledge.[4]

2. A stimulus which, in the past, has consistently occurred in temporal contiguity with a primary reinforcement may also become a satisfier. This, of course, is a statement of the *principle of secondary reinforcement*. Defined in this way, secondary reinforcement represents a learned form of reward. While its phenomena have not been investigated in great detail, it is clear that:

a. Stimuli acquire secondary reinforcing properties by being associated with primary reward (Wolfe, 1936; Cowles, 1937; Ellson, 1937).

[3] The reader should consult Hull's discussion of this point on page 81 of his (1943) book. The entire chapter (VI) on primary reinforcement will repay careful reading, as will, indeed, the entire book.

[4] Some interesting points may be raised in this connection: (1) if a need is reduced, but if the afferent tracts mediating the relevant drive stimulation are blocked, will learning occur? (2) What differentiates drive stimulation from other stimulation? (3) Will the reduction of any stimulus intensity result in learning? (4) If so, is there a threshold of stimulus strength which must be crossed before stimulus intensity reduction will serve as a reinforcement? (5) Are all sensory modalities equal in this respect?

Depending upon the nature of the answers to these questions, it might be possible to define primary reinforcement as the reduction in the number of (weighted) afferent impulses per unit time. Such a conception has a flavor of Guthrie's system about it.

b. The secondary reinforcing properties of a stimulus are lost or extinguished if the stimulus in question is repeatedly presented without primary reinforcement (Grindley, 1929; Bugelski, 1938; Saltzman, 1949).

c. A secondary reinforcing stimulus may reinforce responses in the absence of the drive upon the reduction of which it is based (Estes, 1949).

d. Secondary reinforcement plays an important role in determining a number of other learning phenomena (Denny, 1946; Perkins, 1947; Spence, 1947; Grice, 1948; and Ehrenfreund, 1949).

A number of interesting questions may be raised concerning the mode of operation of secondary reinforcements. Probably the most significant question concerns their fundamental nature; are they conditioned reductions of learned and unlearned drives, or do they represent a type of conditioned confirming reaction (Thorndike, 1935b)? May they be identified with fractional anticipatory goal responses (Hull, 1930), or are these responses and their resulting stimuli merely carriers of secondary reinforcement? Other questions concern the occurrence of "higher orders" of reinforcement and the stimulus generalization of secondary reinforcing stimuli.

3. The reduction in the intensity of a noxious stimulus may be considered as a satisfying state of affairs. The evidence that termination or reduction of noxious stimulation leads to learning is quite extensive. The interpretation of this evidence, however, is not so sure. In animal learning experiments, escape training, and, to a certain extent, avoidance training illustrate the effect of termination of a noxious stimulus upon learning. In general, those acts which have preceded the termination of, for example, a shock tend to be learned.

Perhaps the most important question to ask in connection with this type of reinforcement is this: Is the mechanism of reinforcement based upon the reduction of a noxious stimulus different from the mechanism of primary reinforcement discussed under item 1 above? May not noxious stimulation be considered as a drive state, the reduction of which is satisfying? The acceptance of a theory of primary reinforcement based upon drive-stimulus reduction would

make the identification of these mechanisms a simple matter. This solution, it will be recognized, has been stated by Miller and Dollard (1941), Hull (1943), and Mowrer (1946), and offers an attractive and plausible simplification of reinforcement theory.

4. Some learned responses are drive-producing. When an originally neutral stimulus acquires, through a learning process, the capacity to evoke a response which, itself, gives rise to strong, persistent stimulation, we may speak of a learned or secondary drive. The reduction of such secondary drives may constitute satisfying or reinforcing states of affairs (cf. Thorndike, 1935). Concerning secondary drives, much remains to be learned. The work in this area has been chiefly concerned with learned fear or "anxiety." Thus, an animal is shocked in the presence of a particular neutral stimulus and later shows a tendency to avoid that stimulus (Brown, 1942a, b; Miller, 1944, 1948; Miller and Dollard, 1941; Kaufman and Miller, 1949; cf. also Guthrie, 1938). As such, the stimulus in question becomes classified as an annoyer. However, it is possible to consider the matter in the reverse light, that avoidance or escape from such a stimulus is a satisfying state of affairs.[5]

The evidence for this type of reinforcing mechanism is largely indirect, but is entirely sufficient to warrant the conclusion that the mechanism exists. Briefly, we may mention the work of Mowrer (1939) and his associates on the avoidance conditioned response. Miller (1948) has demonstrated that an instrumental act may be learned on the basis of reinforcement arising from escape from a feared situation. The work of May (1948) and Solomon and Wynne (1950) should also be mentioned in this connection.

5. The possibility remains that some types of stimulation may exist which organisms approach, do not avoid, and may attempt to prolong, but which do not fit into the categories defined under 1 and 2 above. The organisms, in a word, may have innate propensities for certain types of stimuli which are not need-reducing agents

[5] This question is a persistent one and will reappear when we consider the effects of punishment. In this latter case the question may be phrased, does punishment tend to eliminate or "unlearn" the responses which precede it, or is this effect due to the fact that escape from punishment (by doing something else) is rewarding?

and have not been established as secondary reinforcing stimuli (cf. Dollard and Miller, 1950). Sheffield and Roby (1950) have demonstrated that animals will learn to approach certain non-nutritious solutions of saccharin. Similarly, mild cutaneous stimulation, particularly of erogenous zones, may fall under this classification. It may be questioned, on the basis of existing evidence, that such a reinforcing mechanism exists. It should, however, be considered as a definite possibility.

Statement I of the empirical law of effect summarizes observations showing that fixation and elimination are functions of the effects of the acts performed. Statement II goes beyond Statement I in that it characterizes, in a behavioral manner, the effects which lead to fixation as distinguished from those which do not. The breakdown of Statement II into five possible mechanisms of effect goes beyond empirical observation into the realm of theory. Because these statements are more explicit and specific than Statements I and II, they are more subject to correction in terms of future experimentation. It is probable that many revisions of these mechanisms will be required. Certainly, future experimentation of a quantitative nature will allow the valid ones of them to be more precisely stated.[6]

CONDITIONS OF WHICH THE INFLUENCE OF AFTEREFFECTS IS A FUNCTION

Amount of Reward

If reward is influential at all in determining learning, then learning must, of logical necessity, be some function of the amount of reward. When reward is of considerable magnitude, learning occurs. On the other hand, when reward is of zero magnitude, learning ordinarily does not occur and, in the case of already established habits, extinction ensues. Between these extremes must lie some sort of a function.

[6] Even more specific statements, such as the quantitative statements of Hull (1943, 1950), have been made. Such formulations represent the ultimate goal of theory construction, but suffer from the disadvantage of being even more vulnerable to experimental attack. (See Chapter II.)

The simplest conceptualization of this relationship is to hold that reward operates in an all-or-none fashion. This position has been taken by Thorndike (1933b). Under such a view, reward is conceived to operate through a "confirming reaction" or "OK reaction." Variations in amount of reward are conceived to have little effect upon the learning process provided only that the reward is sufficient to elicit the confirming reaction. This view has recently been espoused by Hull (1950). Under these new postulates, Hull regards habit $(_{S}H_{R})$ to be a function of the number of reinforcements (N), the amount of reinforcement per trial being unimportant in this determination.

A number of experiments exist, however, which show that performance increases as amount of reward increases (cf. Grindley, 1929; Gantt, 1938; Crespi, 1942; and Zeaman, 1949). In order to reconcile these findings with the assumption that habit growth is not a regularly increasing function of amount of reinforcement, it is necessary to separate (as intervening variables) habit from performance and to hold that amount of reinforcement determines the latter rather than the former. This is what Hull has done. Performance is held to be a multiplicative function of habit and a number of other factors, of which amount of reward is one. Thus, changes in amount of reward will cause variations in performance without changing the growth of habit.

The alternative view (that amount of reward determines habit rather than performance level) is made explicit in Hull's (1943) earlier treatment of reinforcement theory.

> "In a learning situation which is optimal in all other respects, the limit (M′) of habit strength $(_{S}H_{R})$ attainable with unlimited number of reinforcements is a positive growth function of the magnitude of the agent employed in the reinforcement process."

The facts are not entirely clear. This probably results from the different species, learning situations, and types of reward which have been used in the experiments on this problem. In human learning experiments the results appear to favor the view that reinforcement operates in an all-or-none fashion. Thorndike and Forlano

(1933) find an increasing rate of learning by boys aged ten to sixteen in a multiple choice experiment as the amount of money reward for correct responses increases from 0.1 cent to 0.4 cent each, but no further increase, and even a slight decrement, as the amount is increased from 0.4 cent to 0.8 cent. A similar experiment by Rock (1935), in which the amounts of aftereffect varied within the same series, found that the addition of varying amounts of money to the simple announcements of *Right* increased learning very slightly. In tossing balls over one's shoulder at a target, announcement of *Right* was as effective as this announcement plus money rewards of varying amounts. Eisenson (1935) found that a reward of two cashable tokens leads to somewhat more learning than a reward of one token, though neither was as effective as the announcement of *Right*.[7] Clearly, learning does not always increase with increasing amounts of reward, nor, when it does, is the increase in learning always proportional in a regular way to the amount of the reward.

The result of animal experimentation may be interpreted to support either view of this relationship. In general, there is an indication that performance increases as a negatively accelerated function of amount of reward. Certain irregularities obtained in these experiments, however, suggest that learned factors in the reinforcement process are of considerable importance. For example, Wolfe and Kaplon (1941) found that when four one-quarter kernels of popcorn were given as a reward, more learning resulted than from the giving of a single, intact kernel. This result can no doubt be explained in terms of secondary reinforcement, the activities of the animal associated with the prehension and eating of food resulting in stimuli which have acquired secondary reinforcing properties. More difficult of explanation are the contrast effects obtained by Crespi (1942) and Zeaman (1949). In these cases, learning with a particular amount of reinforcement is carried on until the learn-

[7] It should be noted that Wolfe (1936) found that chimpanzees showed signs of faster learning when rewarded with "high-value" tokens than when rewarded with "low-value" tokens. There is, of course, an obvious analogy between the use of money and cashable tokens in human learning experiments and the use of token-rewards in studies of chimpanzee learning.

ing curve reaches its asymptote. The amount of reward is then changed. Performance increases or decreases depending upon whether the amount of reward has increased or decreased. The change, however, is greater than one would predict on the basis of the new level of reinforcement. This is schematized in Figure 29. Crespi (1944) interprets this effect as meaning that variations

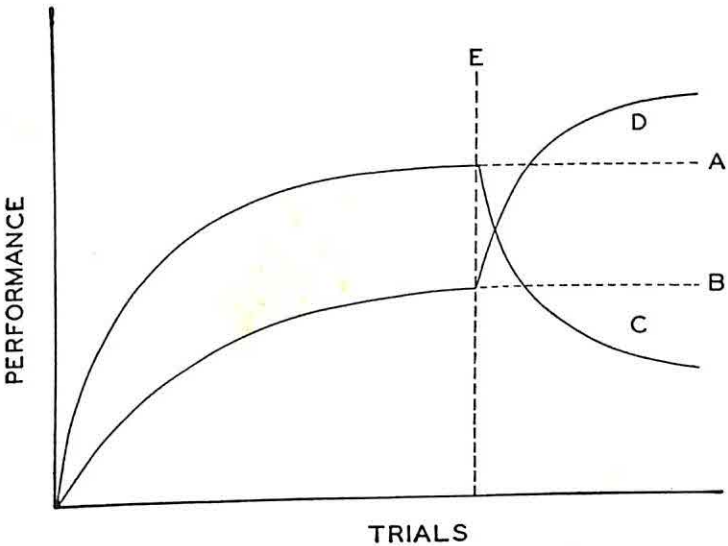

FIG. 29. SCHEMATIC REPRESENTATION OF POSITIVE AND NEGATIVE CONTRAST EFFECTS IN REINFORCEMENT

Curve A represents learning to be expected under a high level of reinforcement and curve B, that to be expected under a lower level. At point E, however, reinforcement levels for the two groups are reversed. Positive contrast effects are represented by D and negative contrast effects by C. These curves do not represent actual data, but are based on the findings of Crespi (1942) and Zeaman (1949).

in amount of reward cause a change to occur in the motivational level rather than in the amount of learning. This means that the organism has acquired habits appropriate to particular amounts of reward (expectations). Increases in amount of reward beyond this "expected" level are supposed to bring about a state of "elation" and to cause disproportionate increase in performance. Similarly,

decreases in amount of reward result in a lowered level of performance through the mediation of "depression." [8]

Another problem in this general area concerns the probable interaction between drive and reward. Thus, one would not expect rewards to be as efficacious in unmotivated as in motivated individuals. Furthermore, there is no particular reason to believe that, as amount of motivation increases, the reward value of a given amount of relevant incentive will increase in a linear, or even in a regular, fashion. There are, however, great technical difficulties involved in studies of this interaction and no very good data exist in solution of this problem.

Frequency of Reward

Reinforcement theories are pretty well agreed that learning is a gradually increasing function of the number of reinforced trials. This assumption underlies the work of Thorndike and the formulations of Hull. Superficially, the facts of conditioning, instrumental learning, and more complex types of learning seem to bear this assumption out. Conditioned responses are typically strengthened by a series of reinforced practice trials while a series of unreinforced trials leads to extinction. Satisfying aftereffects following the same response on successive trials in a multiple choice problem situation are accompanied by progressive fixation of the rewarded response. (Thorndike, 1932, 1935; Lorge, 1933 b; Waits, 1937.) Under these experiments there is a cumulative influence of effect. Experiments on *partial reinforcement,* however, offer an obstacle to the naïve acceptance of this viewpoint. In partial reinforcement experiments it has been noted that, if reinforced and unreinforced trials are interspersed during training, learning proceeds very much as if every trial had been reinforced. This effect was first noted by Pavlov (1927) who found that reinforcing every second or third trial was sufficient to produce learning.

[8] The terms "motive" and "habit" are used here in the sense of intervening variables. Under this conceptualization, Crespi's hypothesis states a law of performance rather than a law of learning.

There are several techniques for studying the partial reinforcement phenomenon. One method, developed by Skinner (1938), consists of giving reinforcements periodically in time rather than giving reinforcement following every response or following a fixed number of trials or responses. This technique is especially well adapted for use in self-pacing instrumental learning situations such as the Skinner box situation (see Chapter III). The general finding is that, under this periodic reinforcement, rate of response increases the more frequently reinforcement is given. There tends to develop a constant number of responses per reinforcement, this index being known as the *extinction-ratio*. It may be assumed that the value of this ratio will depend upon such factors as the strength of drive, the amount and quality of the reinforcing agent, and the amount of physical work performed in making the learned response.

A second, and related, method has been used by Skinner and others (cf. Jenkins, McFann, and Clayton, 1950). Under this procedure, known as aperiodic reinforcement, reinforcements are given at irregular time intervals throughout the learning process.[9]

A third method of studying partial reinforcement is to use the method of Pavlov, that is, to reinforce a given proportion of trials or responses, regardless of how far apart these may be separated in time. In instrumental conditioning situations, the proportion of reinforced responses may be negligible. In fact, Skinner's (1938) results would indicate that learning may proceed with only 1/192 of the responses being reinforced. Furthermore, it would appear that the rate of response rises as this fraction decreases.[10]

Not only may learning be quite efficient under the various types of partial reinforcement, but a number of investigators have reported results that indicate that extinction is slower following partial than

[9] An interesting variation of aperiodic reinforcement may be seen in the experiment of Skinner (1948) on "superstitious" behavior. Here, no response is selected by the experimenter as the one to be learned. Instead, reinforcements are given at random intervals. Learning is manifested in a reduction in the variability of the learner's behavior. The animal tends to learn those responses which have preceded the reinforcement and to respond in these ways for a disproportionate amount of time.

[10] See also the experimental results of Brogden (1939), Brunswik (1939), Cole (1939), Humphreys (1939, 1940, 1943), and Jenkins and Rigby (1950).

following 100 per cent reinforcement. (Cf. Skinner, 1938; Humphreys, 1939 a, 1940; Jenkins and Rigby, 1950; Jenkins, McFann, and Clayton, 1950.)[11]

These results present difficulties to the reinforcement theorist and there has been a general tendency to interpret them as favoring an "expectation" theory of learning.[12] There are, however, several hypotheses which offer potential explanations of these data within the framework of stimulus-response-reinforcement theory.

The paper by Mowrer and Jones (1945) contains a discussion of a suggestion made by Judson S. Brown. According to this view, one difficulty involved in an interpretation of the partial reinforcement effect is that it is difficult adequately to identify the learned response. In the lever-pressing situation, for example, it is customary to speak of each depression of the bar as a (or rather *the*) response. When reinforcement is given only on alternate bar depressions, however, there is some justification for thinking of the learned response as being *two* bar depressions, and for considering that this pattern of movements is learned as a single response or act. If reinforcement is given only after fifty bar depressions, the animal may be learning the act of jiggling the bar rapidly for a period of time. Similarly, during extinction, the number of unreinforced trials should be counted as the number of unreinforced response units rather than as the number of bar depressions per se. Viewed in this way, the acquisition and extinction data of the partial reinforcement experiments do not appear to be so strikingly at variance with other learning data. The results of Mowrer and Jones (1945) offer support for this explanation. When measured by number of bar depressions, resistance to extinction decreased as an approximately linear func-

[11] That this may be true even when partial reinforcement has produced less learning than 100 per cent reinforcement is suggested by the results obtained by Finger (1942a, b). The operation of secondary reinforcement and other factors make Finger's results difficult to interpret, however. (Cf. Lawrence and Miller, 1947.)

[12] Humphreys (1939a, 1940, 1943) interprets his findings in this way. In his recent book, Hilgard (1948) also appears to regard this as evidence which favors the general viewpoint of Tolman. However, the apparent advantage of an expectation theory in this matter lies mainly in its vagueness and in the *ex post facto* nature of its predictions concerning expectancy. Sheffield (1949) has attacked the expectancy point of view on these grounds.

tion of the proportion of reinforced trials. Thus, extinction was slower following partial than following 100 per cent reinforcement. On the other hand, when resistance to extinction was measured by the number of *response units* (two pressings of the bar considered as a single response unit in the case of 50 per cent reinforcement), resistance to extinction increased as a function of the proportion of reinforced trials. This latter effect is probably due to the fact that more work is involved in making a response unit under partial than under 100 per cent reinforcement, the same relationship holding true for extinction.

A second type of explanation has been formulated in terms of the principle of secondary reinforcement. According to this view (Denny, 1946), stimuli associated with the correct response acquire secondary reinforcing properties which serve to strengthen the making of that response when primary reinforcement is absent. Denny's results, obtained with rats in the T-maze situation, clearly indicate that the factor of secondary reinforcement can operate to produce the partial reinforcement effect.

Hull (1941) and Miller and Dollard (1941) have proposed an additional explanation for the slower extinction rate following partial reinforcement. This hypothesis is based on the fact that, during learning under 100 per cent reinforcement, one of the cues associated with the correct response is the stimulus trace of the reinforcement given on the previous trial. During extinction, these cues are missing because no reinforcement has been given on the preceding trials. When learning is carried out under conditions of partial reinforcement, however, many of the correct responses are made in the absence of any cues resulting from previous reward. During extinction, then, the conditions of stimulation resemble those present during acquisition more closely than in the case of the 100 per cent reinforcement group. Faster extinction following 100 per cent reinforcement thus occurs, owing to a relative failure of stimulus generalization to occur. One deduction which can be drawn on the basis of this hypothesis is that the relative advantage of partial reinforcement (with respect to resistance to extinction) should be greater under massed than under distributed practice during ac-

quisition. This is because spacing of trials would allow the stimulus traces from the previous trials to dissipate, hence this difference between partial and complete reinforcement would be negated. Inferential support for this hypothesis may be obtained from a number of experiments, but the definitive experiment by Sheffield (1949) clearly indicates the importance of this factor and gives powerful support to the Hull hypothesis.

Whether or not these explanations will, singly or in combination, account for all of the partial reinforcement data cannot be stated at the present time. A great deal of additional experimentation is required in this area. The explanations are promising, however, and are supported by a considerable amount of data. It would appear to be premature, on the basis of the present findings, to conclude that the partial reinforcement data can be accounted for only in terms of an expectancy principle.

Delay of Reward [13]

Studies on the delay of reward have revealed the empirical fact that those responses which occur in close temporal contiguity to a reinforcing state of affairs tend to become more strongly learned than those which are more separated from the reinforcement in time. With infinite (or very long) delay of reward, extinction rather than learning occurs. There is implied, then, a gradient of delay of reward or a functional relationship between amount of learning and immediacy of reward. In most studies of this relationship, the experimenter has allowed the to-be-learned response to be made and has then administered reinforcement after a specified period of time. Thus, the gradient of delay which is usually studied is the backward one, that is, with the response preceding the reinforcement. On the other hand, the results of Muenzinger, Dove, and Bernstone (1937), Jenkins (1943a, b, c), Thompson and Dove (1943), and some of the results on the spread of effect would seem to indicate that the gradient of delay also exists in the other direction, that is, with the reinforcement preceding the to-be-learned

[13] See also the section on Spread of Effect.

response. From the studies on delay of reward, certain general trends are apparent: the quantitative nature of the delay gradient has become specified with greater and greater precision, and the nature of the delay effect has become increasingly plain. As the understanding of the nature of the delay effect has grown, perhaps the study of the quantitative nature of the delay gradient has become somewhat less important.

The pioneer study on delay of reward was conducted by Watson (1917). His study and the later one by Warden and Haas (1927) failed to reveal any differential effects of delayed vs. immediate reward. In both cases, however, the animals were prevented from eating for the period of delay *in the presence of food.*[14]

Later studies consistently noted the positive relationship between immediacy of reward and amount of learning. Furthermore, the general quantitative form of the relationship became increasingly clear: that amount of learning is a negatively accelerated decreasing function of the amount of delay.[15] This was formalized by Hull (1932) in a statement of his well-known *goal gradient hypothesis* and has received extensive treatment in Hull's *Principles of Behavior* (1943).

When the earlier work on delay of reinforcement is compared with the later studies, it can be seen that there has been a trend toward finding shorter and shorter intervals of permissible delay. Thus Watson (1917) found a delay of thirty seconds to result in as much learning as immediate feeding and Warden and Haas (1927) found no appreciable decrement from delays up to five minutes in length. Later work, however, showed that the period of delay beyond which learning might be demonstrated is very much shorter than this. The reason for this difference in results is, undoubtedly, the fact that the earlier investigators did not control the factor of secondary reinforcement. Both Watson and Warden and Haas

[14] Other early studies on delay showed differential learning to result from delayed and immediate reward. These include studies by DeCamp (1920), Kuo (1922), White and Tolman (1923), and Sams and Tolman (1925).

[15] Some of these studies include Clements (1928), Hamilton (1929), Yoshioka (1929), Dennis (1935), Anderson (1932), Wolfe (1934), Grice (1943), Perin (1943a, b), and Thompson (1944).

delayed the animals in the feeding chamber, food being present but in a covered food dish. The presence of this food served to reinforce the correct response secondarily (cf. Thorndike, 1932). It is reasonable to suppose that secondary reinforcement also played a major role in other early experiments. Perin's (1943a, b) results indicated that, when some of the secondary reinforcing cues were absent, the period of permissible delay for the white rat scarcely exceeds thirty seconds. (Cf. also Perkins, 1947.)

In his 1943 treatment of this problem, Hull broke down the delay phenomenon into two parts. The first of these he termed the *gradient of reinforcement* after Miller and Miles (1935). This term he used to denote the relatively short gradient resulting from the later experimental work, particularly that of Perin (1942a, b) which, he felt, was free from the complications introduced by secondary reinforcement and, therefore, represented a basic characteristic of the organism. The longer gradient, which combines this gradient of reinforcement with the operation of secondary reinforcement, Hull continued to call the *goal gradient*. Spence (1947), in an extension of this general approach, proposed that all delayed reward learning be considered as an effect of secondary reinforcement, and that there is no "primary" gradient of reinforcement. He also pointed out possible sources of secondary reinforcement in the Perin experiments which might account for the delay gradient which Perin obtained. Particularly, he noted that proprioceptive stimuli might acquire secondary reinforcing properties. In the definitive study of delay of reinforcement, Grice (1948) undertook further to reduce the possible differential effects of secondary reinforcing stimuli, including proprioceptive stimuli, which might acquire secondary reinforcing properties. That he was relatively successful in this may be seen in Figure 30, which compares the results obtained by Perin and Grice. It will be noted that the findings of Grice drastically reduce the amount of permissible delay. This result lends powerful support to Spence's (1947) hypothesis. What little learning accompanies delay in Grice's experiment may probably be attributed to residual differential secondary reinforcement.

This series of experiments culminating in Grice's (1948) findings

makes it possible to interpret all delay of reinforcement learning in terms of the operation of secondary reinforcement. Since amount of secondary reinforcement may be manipulated independently of the temporal factor, much of the discussion of the shape of the goal gradient function, whether it is exponential or logarithmic in form, becomes futile. It is also worth noting that Spence (1947) and Grice

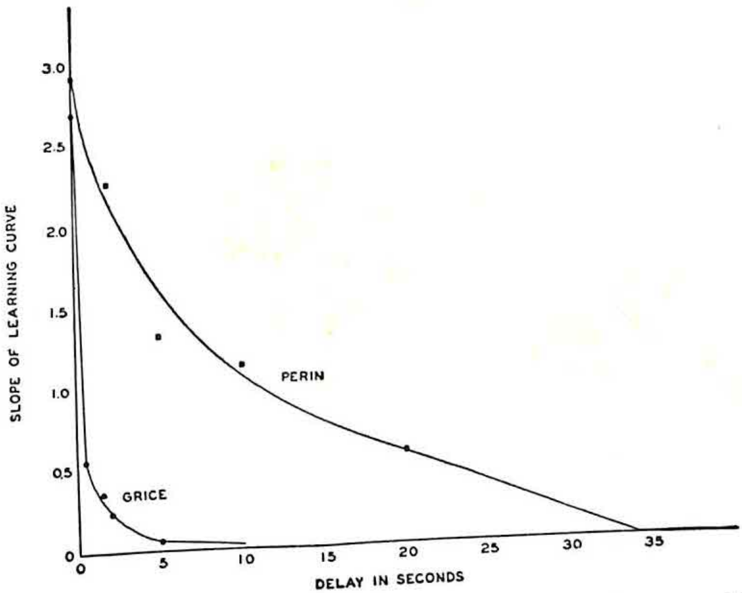

FIG. 30. RATE OF LEARNING AS A FUNCTION OF DELAY OF REWARD IN PERIN'S (1943) AND GRICE'S (1948) EXPERIMENTS

(From Grice, *J. exp. Psychol.*, 1948, 38, p. 13)

The steeper gradient obtained by Grice is due to the greater elimination of secondary reinforcing stimuli in the Grice experiment.

(1948) have demonstrated that the retroactive action of reward is not such a potent objection to reinforcement theory as it was once thought to be.[16]

Most of the work on amount, frequency, and delay of reward

[16] Postman's recent review of the literature on the law of effect (1947) contains a discussion of these objections to reinforcement theory. It may also be noted, in passing, that in his recent set of postulates, Hull (1950) considers delay of reward to be a determiner of performance rather than of learning.

has been conducted with animals. Relevant information from human learning experiments on these matters is scanty. Furthermore, great difficulties confront investigators of these phenomena at the human level owing to the rich context of previous learning which the human subject brings to the experimental situation and because of the mediation of language. It should not be imagined that the findings of animal learning can be applied directly and without change to the control of human learning. On the other hand, it is improbable that basically different mechanisms occur in human learning. In fact, in the learning of very young children, in the learning of motor skills, and in the unverbalized learning of emotional habits it would not be surprising to find that these principles apply directly.[17]

Spread of Effect

There is another empirical phenomenon of effect which Thorndike, who first announced it (1933a, c), regarded both as strong proof of the strengthening influence of satisfying aftereffects and as comprising important new knowledge concerning its mode of action. In characteristic experiments, Thorndike gave subjects a series of situations to which a number of responses were possible. Following each response, the experimenter could announce either *Right* or *Wrong.* In one such experiment, the subject was given a long list of stimulus words, one at a time, with the instruction to respond to each by saying any number between 1 and 10. Under these circumstances, the experimenter can arbitrarily decide to reward the responses to certain stimuli (regardless of the response made) and to punish responses to certain other stimuli. In this manner various patterns of rewarded and punished connections may be established. For example, the pattern WWWWRWWWW may be given. The general result was that the occurrence of *Right* as a consequence of a response not only strengthened the association it followed, but also, and to a smaller degree, it strengthened the wrong associations which preceded and followed the rewarded responses. This result

[17] Dollard and Miller in their new book, *Personality and Psychotherapy* (1950), should be consulted in this connection.

may be represented by the double gradient shown in Figure 31. The phenomenon is known as the *spread of effect*.

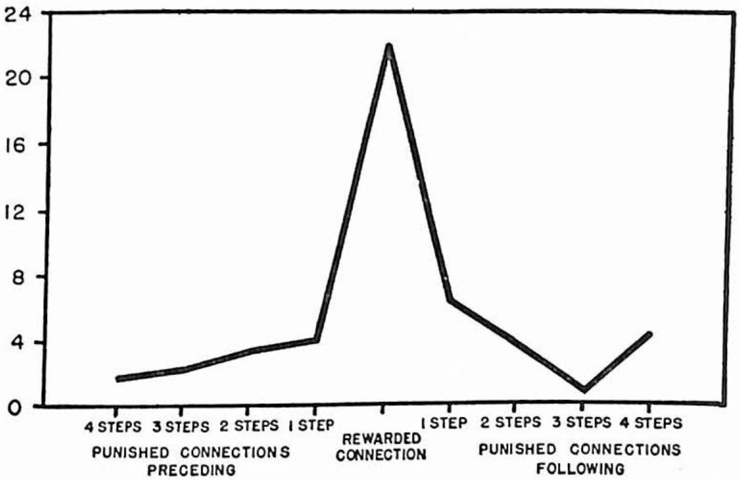

FIG. 31. THE SPREAD OF EFFECT
(From Thorndike, *An experimental study of rewards*, p. 55)

This phenomenon has been noted by other experimenters (cf. H. Brandt, 1935; Muenzinger and Dove, 1937; Farber, 1941). As it has been observed, spread seems to be limited to a few steps or discrete associations on either side of the rewarded response. The number of such steps appears to be more important for fixation than the time intervening between the rewarded response and the punished ones on either side, although the time intervals which have been used have been relatively short (as from two seconds to twelve seconds). Neighboring responses which are neither rewarded nor punished (that is, after which no consequence appears) are strengthened by spread of effect at least as much as are punished ones (cf. also Thorndike, 1934). Spread may occur when the satisfying aftereffect is not specifically relevant to, or does not belong to, the response it follows (Thorndike and Lorge, 1935). Moreover, it seems not to be a gradient of uncertainty such as might occur if the subject but vaguely recalled the place at which the word *Right* had been spoken and tried to repeat his responses in that part of the series (Muenzinger and Dove, 1937).

On the other hand, a number of recent experiments have cast some doubt upon the existence of the spread of effect phenomenon. This evidence has been of two kinds: a failure to obtain spread of effect, and an attempt to explain the spread phenomenon in terms other than strengthening-though-reinforcement. One criticism of Thorndike's work is that he failed adequately to allow for the subject's tendency to repeat certain responses regardless of reinforcement. For example, the assumption that a subject will respond one-tenth of the time with a particular one of ten responses may or may not be correct since the subject does not respond in the entirely random and independent fashion of tossed coins or rolled dice. Tilton's (1939, 1945) work attempted to take this and other factors into consideration. His experiment fails to demonstrate spread of effect in the usual sense, although there is a suggestion that responses preceding and following rewarded responses tend to be repeated more frequently than they would have been had the reward not been given to the adjacent response.

Zirkle (1946a) has obtained results which indicate that spread of effect is not due to a strengthening of the stimulus response connections which lie adjacent to a rewarded response. This follows from his finding that when the order of the stimulus items is changed from trial to trial the spread of effect still appears in the appropriate serial positions, but not for the appropriate stimulus items.[18] This finding indicates that the spread phenomenon may be located in the response series rather than in the series of stimulus-response connections. This indication is made more apparent by the results of Jenkins and Sheffield (1946) who found that the spread phenomenon is obtained only when the correct response is repeated. In explanation of their results, Jenkins and Sheffield offer what they term a guessing-sequence hypothesis. Reasoning from the results on spontaneous alternation (cf. Heathers, 1940) and results obtained by Goodfellow (1938), Skinner (1944; cf. Solomon, 1949), and others, they point out that if the subject repeats the correct response

[18] Zirkle interprets the tendency to repeat the *rewarded* connection as an example of the von Restorff effect and has obtained data which substantiate, to a degree, this hypothesis (1946b).

he has an increased tendency to make some *other* response to the immediately succeeding items. If the number of available responses is finite (as it is in the usual rational learning situation where the subject responds with a limited series of numbers) this tendency toward alternation reduces the number of available responses which can be made to the succeeding stimulus. As a result, there is an increased probability that the subject will respond with the previously given "incorrect" response.[19] Such an explanation of the spread of effect phenomenon, obviously, depends upon a repetition of the previously reinforced response. The hypothesis is supported by the fact that the spread phenomenon occurs most frequently in cases where the correct response is repeated. It is also supported by the asymmetrical nature of the spread gradient, which typically shows a greater effect following than preceding the rewarded response.[20] The guessing-sequence hypothesis has been elegantly stated by Smith (1949) in terms of probability theory. His experimental results and those of Sheffield (1949) show gradients resembling those of the spread phenomenon under conditions of no learning where, presumably, the Thorndike effect could not be found.

It may be questioned, then, whether the spread of effect phenomenon is not merely an artifact of the experimental methods employed in studying it. This is by no means certain, although we must regard the existence of the phenomenon as suspect until such time as it may be demonstrated under circumstances where the objections to it do not apply. It may well be that such a demonstration cannot be made.

It has been stated that the existence of the spread of effect phenomenon is crucial to the theoretical position of Thorndike,

[19] Jenkins and Sheffield (1946) also note a tendency for successive responses to be chosen from the same general range of numbers, a tendency which would enhance the effects of alternation noted above.

[20] The theory has some difficulty in explaining why there should be *any* spread to the items preceding the rewarded response. Results obtained by Zirkle (1946b) as plotted by Hilgard (1948) show spread only to the succeeding items. (Cf. Jenkins and Cunningham, 1949.) The asymmetrical gradient, it should be noted, can be interpreted in Thorndikian terms in view of the fact that the reward follows the correct response and, hence, is closer to the succeeding than to the preceding items in time.

and, by implication, to reinforcement theory in general (Hilgard, 1948). This is not necessarily true. It is still possible for reinforcement to strengthen specific connections (all experiments on the spread of effect phenomenon find that the rewarded response tends to be strengthened) even if this effect does not spread to adjacent items. We have already seen, in the case of the delay of reward studies, that the analogous delayed reward learning phenomenon may also be an artifact in the sense that it is produced by secondary reinforcement. None of the spread of effect studies, however, have been designed in such a way as to permit the differential operation of secondary reinforcement to any great degree. If a method could be devised for giving secondary reinforcement to the connections adjacent to the rewarded item, doubtless spread of effect could be obtained, although the significance of such a demonstration would be limited. As has already been noted, the absence of a "primary" gradient of reinforcement or of a "primary" spread of effect phenomenon serves to negate one criticism, that of the retroactive operation of effect, which has often been made of reinforcement theory.

Punishment

Thorndike's original statement of the law of effect (1911) held that rewards and punishments influence learning in opposite ways. Reward, following a stimulus-response connection, it was held, would promote learning. That is, given the situation again, that response would be more likely to occur. If punishment followed the stimulus-response connection, however, the opposite result was supposed to follow. Thus, reward caused learning and punishment caused unlearning. Later, however, Thorndike changed his position with respect to punishment (1932). In this latter statement of the law of effect, the general function of reward remained unchanged, but punishment was held to be followed by little, if any, unlearning. Thorndike and his associates have devoted a considerable amount of research to this problem.

In a typical experiment, Thorndike (1932) asked subjects to guess which one of five English words gave the meaning of the Spanish

word at the left of the line. They did this repeatedly with 200 such lines as the following:

Abedul: ameer . . . birch . . . couch . . . carry . . . punch

If the word chosen and underlined was correct, the experimenter said *Right;* if it was incorrect, he said *Wrong.* It was then possible to discover the relative influence of the two different aftereffects by counting the number of times that a correct response was followed on the next trial by that correct response or by a wrong one, and the number of times that an incorrect one was followed by any other response or by the same wrong one. The results were corrected for the number of right and wrong responses to be expected by chance—that is, by subtracting 20 per cent from the obtained number of right responses and 80 per cent from the obtained total of wrong ones (or 20 per cent for each wrong one). If hearing *Wrong* spoken by the experimenter as an immediate consequence eliminates directly, the response it follows should be weakened, as is evidenced by a shift to some other response on the following trial, whereas if hearing *Right* fixates, the response it follows should be repeated on the next trial.

The consequence, *Right,* is found to strengthen, as Thorndike's other experiments would lead one to expect, but the consequence, *Wrong,* has little, if any, weakening influence. Indeed, Thorndike concludes that a response gains more in strength from occurring than it loses from being followed by the word, *Wrong.* As he was careful to point out, this does not mean that punishment does not influence learning, but only that it does not do so by directly or intrinsically weakening the "punished" response. This failure of *Wrong* (and certain other annoying aftereffects) to weaken the response it follows has been verified in somewhat different ways by a number of investigators.[21]

The conclusion that, under the conditions of these experiments,

[21] Thorndike (1935b, 1947), Lorge and Thorndike (1933), Lorge (1933a, b), Tuckman (1933), Lorge, Eisenson, and Epstein (1934), Proshansky and Murphy (1942), Nuttin (1947a, b), and Stone (1948). Stone (1950) has recently published a cogent review of the literature on this subject.

annoying consequences are not the opposite of satisfying ones is of the first importance. The generality of this conclusion needs to be known. There is, first of all, the question of the legitimacy of assuming, in experiments such as the one with the five English meanings for each of a list of Spanish words, that by chance 20 per cent of the subject's choices would fall on each possible word. If this assumption is invalid, Thorndike's correction for chance is inadequate. He has recognized this possibility and has argued from the evidence of his data that the correction is legitimate. Hull (1935b) contends, however, that the distribution of chance choices will not be equal, but that there will be a bunching on one or two. Experiments in which the influence of *Right* and *Wrong* is measured from the empirical base of actual repetition without informing aftereffect, instead of from the chance base of an expected 20 per cent of each alternative, have yielded conflicting evidence. Stephens (1934a, b) and Tilton (1939) have obtained results which supposedly indicate the weakening effect of punishment when an empirical base is used. On the other hand, Stone (1948) has obtained results which fall in the Thorndikian tradition. Furthermore, there seems to be some doubts about the adequacy of the procedures employed by Stephens (1934a, b) and Tilton (1939). (Cf. Stephens and Baer, 1937, and Stone, 1950.)

Even if Thorndike's findings are accepted as being methodologically adequate, however, a difficulty remains. A great number of experiments have indicated that punishment (for errors) causes an increase in the rate of learning. Indeed, a number of investigations have indicated that non-specific punishment has a facilitating effect, and there is even some evidence to show that punishment for making the correct response may be facilitative.[22] In fact, the results which have been obtained on the effects of punishment are so

[22] Far too many studies are concerned with these effects of punishment to be reviewed here. In the matter of non-specific punishment, the work of Bunch (1935), Gilbert (1936, 1937), and Bernard (1941, 1942) should be mentioned. Muenzinger's work on this problem and on the matter of punishment for correct responses should also be noted (1934, 1946). For additional references the reader is referred to the recent, exhaustive review of the literature on the law of effect by Postman (1947).

diverse and extensive that probably no general conclusions can be drawn on the basis of existing information. Part of this confusion lies in the use of the generic term, *punishment*, to describe a number of widely different states of affairs, all of which are unquestionably annoying under the general definition proposed by Thorndike. However, when a child touches the hot stove or when a rat crosses a charged grid, reactions may be observed which appear to be quite foreign to the behavior of the college sophomore who is told that his guess of an English equivalent has been *Wrong*. In general, it would appear that three general types of punishment have been used in learning situations. These are (1) painful stimulation, as by an electric shock, (2) frustration, as by delaying reinforcement or by making the task more effortful, and (3) the giving of information, as by saying "Wrong." It is apparent that these types may overlap to some extent, as for example when an electric shock serves an informative function. On the other hand, there is no particularly good reason for thinking that these types of punishment all produce the same effects upon the learner or upon the learning process.

In addition, it should be noted that a number of mechanisms exist (and are permitted to operate in many experiments) through which punishment may influence the learning process. These mechanisms may operate even though punishment *does not* have a direct weakening effect upon stimulus-response connections. Some of these mechanisms may be briefly noted. Other similar mechanisms are felt to be less important or less pervasive in their influence, and still others doubtless remain to be identified.

(A) *Motivation.* The use of punishment may increase the general motivational level of the learner. This motivational effect should not operate differentially in the case of correct and incorrect responses, but it should serve to enhance all learned performances and (at least, under the theoretical assumptions of Hull) should magnify differences in habit strength which may exist. Thus, a small difference in habit strength between the correct and incorrect response may not be manifested in behavior when drive is low. With a high motivational level, however, the response with the stronger habit strength should tend to predominate. In addition, an increase in

motivation may serve to activate and sustain the general activity level of the learner and, thus, to make more probable the discovery of adequate ways of behaving.

(B) *Learning to do something else.* When a response is punished, if the learner is allowed to terminate the punishment by doing something else, the latter response is likely to be learned. This is because escape from the punishment serves as a reinforcing state of affairs. For example, if in the maze situation, the learner enters a cul-de-sac where he receives a strong electric shock, and if the learner is allowed to terminate that shock by withdrawing, the rewarded act is that of leaving the blind alley. This response, upon being learned, may become anticipatory and eventually result in complete avoidance of the cul-de-sac.

(C) *Better stimulus reception.* In many cases, the use of punishment places the learner in a conflict situation. Consider the experiments reported by Muenzinger (1934) and Muenzinger, Bernstone, and Richards (1938) wherein the learner (a rat) received electric shock immediately after choosing the correct side (the one baited with food) in a discrimination learning situation. The animal, after a few trials, is faced with an approach-avoidance conflict situation (hunger vs. fear). (Cf. Brown, 1942a, b, and Miller, 1944.) Under these conditions, hesitant behavior will be observed. During this period of hesitation, however, the relevant stimuli are present. Under these circumstances, the use of punishment may cause the learned better to receive the relevant cues. This effect of "caution" is discussed at some length by Muenzinger (1946) who, also, has obtained independent verification of this hypothesis. (Cf. Muenzinger and Fletcher, 1937).

(D) *Learned avoidance due to fear-reduction.* Use of painful stimulation as punishment results in the learning of a fear response (Miller and Dollard, 1941). This fear is elicited by stimuli which have accompanied the painful stimulation and is reduced by the removal of these stimuli. If an individual is punished for making a certain response, on subsequent occasions, the incidental stimuli associated with that response, including the initial stages of the response itself, arouse this learned fear. If the individual now

behaves in such a way as to remove himself from the fear-producing stimuli, this response will be reinforced by fear reduction and will, consequently, be learned (Miller, 1948). The learned response, in this case, however, is the one which reduces the fear, not the one which causes it. Thus, by this mechanism, punishment can cause the elimination of learned responses. There is, of course, no guarantee that the new response will be one which will be counted "right" by the experimenter, except that in most learning situations only a limited number of responses are available.[23]

It will be noted that the operation of any one of these mechanisms could produce an important effect upon learning, yet no one of them runs counter to Thorndike's (1932) hypothesis that punishment exerts no direct weakening effect upon the connection it follows. All of the mechanisms discussed here are based upon the assumption that "incorrect" responses may be, and often are, eliminated by a process of competition with other, stronger, responses. The first mechanism deals with the problem of competition of response directly, while the other three describe ways in which punishment may strengthen the correct or some other competing response at the expense of the incorrect response. This, of course, does not prove that Thorndike's hypothesis is correct. It is offered in an attempt to give some basis for a reduction of the confusion which now exists in this field of study. It is also offered with a note of caution against the naïve acceptance or rejection of Thorndike's hypothesis on the basis of results obtained in an area which is so filled with experimental complexities and theoretical possibilities.

Information and Effect

In many studies of human learning, information (knowledge of results) has been the source of reinforcement. Such information may be inherent in the learning task itself, as when the subject observes the results of his own behavior in the pursuitmeter situation, or the

[23] In "free" learning situations, of course, learned fear-reducing responses can be anything but "correct" in this sense, as witness the learning of many neurotic symptoms, which may be based on this type of reinforcing mechanism.

information may be given by the experimenter by means of some signal. Usually, information depends upon past experience and transfer of training, since the subject must be able to interpret the informative cues. The use of information also depends upon the subject's motivation, at least to a certain extent. Thus, the learner must want to make the correct response before the informative signals of *Right* and *Wrong* can have much efficacy.

Information can vary in degree of specificity or precision. In a repetition of one of Thorndike's line-drawing experiments (1927), Trowbridge and Cason (1932) have found that learning increases with increasing precision of the information given. The subjects drew only three-inch lines, but drew them under four different conditions in a counterbalanced order.[24] The conditions were: (1) the control, in which the experimenter said nothing after the drawing of each line; (2) a nonsense-syllable condition, in which the experimenter spoke a different nonsense syllable when the subject had finished drawing each line; (3) a "right" and "wrong" condition, in which the experimenter announced *Right* or *Wrong* after each drawing (as Thorndike had done); and (4) an exact information condition, in which the subject was told the amount and direction of his error.

The major results may be seen in Table XIV. There is no learning under the control condition and none when a nonsense syllable is spoken after each line. Announcement of *Right* or *Wrong* leads to a sharp reduction of error, but exact information about the amount and direction of errors both minimized them early in practice and reduced them as practice went on. The percentages of correct responses (not given in the table) for the four conditions are: control, 13.6; nonsense-syllable, 5.1; Right-Wrong, 22.6; and exact information, 54.8. These percentages corroborate the average error scores in demonstrating that increased precision of information about the results of responses is accomplished by increasing amounts of learning.

This is the usual result. Without information, little or no learning

[24] The use of a counterbalancing technique in this situation is probably not legitimate. See the discussion of this in Chapter I.

results. On the other hand, when information of results is given, learning occurs and becomes more efficient over wide limits as the precision of the useful information increases. The more exact the information, the more exact can the standard become and the more precise may be the selection among responses. It may be questioned whether *all* information about the results of an act can be excluded from a subject as complexly motivated as the human being. Information concerning the relation of an act to the experimenter's standard can be excluded, but the subject may still obtain some information about the relation of the act to a standard of his own. In estimating the lengths of strips of paper without information from the experimenter, he may form an opinion about the accuracy

TABLE XIV

AVERAGE ERRORS IN UNITS OF ⅛ INCH WHEN EACH CONDITION APPEARED IN THE INITIAL POSITION OF THE PRACTICE ORDER

(From Trowbridge and Cason, *J. gen. Psychol.*, 1932, 7, p. 253)

Groups of Trials	Control	Condition Nonsense	Right-Wrong	Exact Information
First 30	5.97	8.87	7.33	1.93
Middle 40	6.85	8.57	5.35	1.12
Last 30	6.40	8.43	3.90	.97

of his estimate, which then functions as if it were information. In drawing lines he has kinaesthesis, on the basis of which he can estimate the relation of the line drawn to his own conceptual standard of the length of line he tried to draw. Some of Thorndike's (1932) results on line-drawing without formal information of results show an approach to a stable value which presumably represented the subject's own standard which he approached with the aid of informal information available to him (cf. Seashore and Bavelas, 1941).

Information concerning the relations between the results of an act and some standard which the subject is motivated to attain may be regarded as synonymous with effect. Announcement of *Right* by the experimenter, or any stimulus which informs the subject that he has done what he was motivated to do—to hit the target,

to keep the stylus on the disk of the pursuitmeter, to anticipate the next word in the list correctly—is, at the same time, information and effect. Each is a consequence of response; each states for the subject, however roughly, the degree of congruence between his motivating conditions and the results of his behavior. Each tells him, that is, to what extent the motives which led to the act, or any other motives functional at the time of the consequence, have been satisfied. There seems to be no need to make a distinction between information and effect in the present context.

The term, information, does not imply awareness by the subject of the relations between motive and consequence, or even of the nature of the consequence, or of what is being learned. He may be aware of one or more of these or he may not. It implies only a condition of stimulation which fits, or is congruent with, the motives of the individual to some degree.

In consideration of the fact that so many of the human educative procedures depend upon this form of reinforcement, it is unfortunate that there is not a greater amount of systematic data concerning the nature and functioning of information. The conclusion, derived from the general principles of learning, is that, for most efficient learning, knowledge of results should be administered as quickly and as specifically as possible.[25] It is probably this factor, more than any other, which makes individual instruction more effective than group instruction. Too often, students receive information concerning their performance only infrequently. Moreover, this information is often given in the manner of relatively unprecise letter or number grades. Learning will be most rapid under conditions where information is given frequently and specifically so that the student may identify his appropriate and inappropriate responses. Furthermore, it is important that the information which is given be relevant to some motive of the student. As Dollard and Miller (1950) have pointed out, many lower-class children do not profit greatly from

[25] One of the authors recalls that, during the recent war, instruction in a certain type of shooting suffered from the fact that information concerning how well the student had shot was not given to him until several days after firing, and then only in an unprecise fashion. It was not surprising that, under these circumstances, no demonstrable learning occurred.

ordinary school grading systems since they have not been trained to want to receive good grades.

Learning without Awareness

The law of effect does not require that the learner be aware of his motivation, the reinforcing states of affairs, or the responses he learns. These factors operate in a relatively automatic fashion without necessarily producing verbal accompaniments. If we use *ability to report* as a criterion of awareness, we may say that subjects may learn without awareness of any of these factors. (Cf. Freud, 1933; Guthrie, 1938; Dollard and Miller, 1950.) Learning without awareness is supported by many phenomena of the laboratory and of daily life.

Subjects practicing a maze may enter blind alleys and later eliminate them, for example, without being able to report that they have done so. Subjects learn successive lists of words faster, as a result of practice, without being able to report what methods they are using to enable the faster learning—methods which they have learned, nevertheless, during the learning of the words. In like manner, the operation of much transfer is unconscious, the subject being unable to report what is influencing the learning of a second activity in a given way, when experimental analysis reveals that it is the retained effects of an earlier learned activity. Similarly, clinical evidence points to the fact that many early experiences influence later behavior in a profound manner even though (or perhaps, because) the individual is unaware of this previous learning. In learning complex acts of skill, one is typically unaware of precisely what specific responses have been learned. A better golf or tennis stroke may be fixated without the individual's awareness of the detailed pattern of learned responses by which it has been executed.[26] Conscious analysis often follows rather than precedes learning (cf. Perrin, 1921).

[26] One reason why it is more difficult to teach certain motor skills than it is to teach verbal skills is that we have no vocabulary to describe the component parts of the skill. Thus, the learner is unable accurately to report what he is doing, and the instructor is unable to tell him how his performance may be

Learning without knowledge of the specific characteristics of what is being learned—that is, without knowledge of the component acts in the total organization which produces the final desired result—is, in fact, so frequent a characteristic of learning as scarcely to need further proof. Additional evidence is furnished, however, by a number of Thorndike's experiments (1932) in which the correctness of the response was a function of some feature utterly aside from the characteristics of the response itself. Thus, in an experiment on learning the correct meaning among five possible ones for a given word, the correct word appeared zero times in the first position at the left, ten times in the second, twenty in the third, thirty in the fourth, and forty in the last to the right. Would subjects trained on such series with appropriate announcements of *Right* and *Wrong* learn this pattern of correct responses and exhibit it on a later test, in spite of the fact that they were unaware of the existence of the pattern during the training? Whether they had noticed the pattern was discovered by questioning, and conclusions were drawn only from the data of those who reported not having noticed that there was any relation between position and frequency of correctness. Those subjects who had been unaware of this relation had learned it nevertheless, as shown by the pattern of their responses on a test. (Cf. also the similar studies of the conditioning of verbal expectations by Humphreys, 1939b.) That language patterns themselves may be learned without awareness of that learning is indicated by an experiment conducted by Greenspoon (reported in Dollard and Miller, 1950). Greenspoon interviewed subjects in either one of two ways. In one group, whenever the subject used a

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improved. We have words to describe certain overt acts, usually of a rather gross character. These words become attached to the appropriate behavior so that when the verbal cues are given, the response regularly follows. Thus a child may be taught some acts, such as to open an automobile door, simply by saying once, "Pull back on the handle." Since the child has already attached appropriate responses to the verbal cues "pull back" and "handle," transfer of training operates to produce the new response immediately. On the other hand, there are no words to describe the pattern of muscular contractions involved in throwing a baseball and many thousands of trials are required to learn this skill to even a mediocre level of proficiency. Once he has learned this skill, moreover, the child will be unable to tell you how he does it, except to say that he "just throws it."

plural noun, the experimenter said "Mmm-hmm."[27] In the other group of subjects, whenever a plural noun was used, the experimenter remained silent. The results show an increase in the percentage of plural nouns used by subjects in the first group but not by subjects in the second. The subjects who learned to say plural nouns were evidently unaware of the reinforcing function of "Mmm-hmm" and of the habit they had acquired.

Other experiments support the same conclusion, that learning may occur when the subject is not aware of what he is learning or of the fact that reinforcement is acting upon his responses.[28] This conclusion is inferentially supported by the empirical operation of reinforcement in animal learning, where the existence of awareness is improbable. Learning in early infancy and the learning shown by vegetative aments also supports this conclusion. It is probable, however, that over a wide range of conditions, rate of learning by human subjects is increased by knowledge of what is being learned and the consequences of the responses being made. This increase with awareness is most probably due to language-mediated transfer of training.

The fact that learning can and does occur without awareness is important, however, in more general ways than the evidence directly states. In the light of it, incidental learning is easier to understand as being actually a function of motive and effect. The possible limitation on the generality of the law of effect, that subjects are very often unaware of their motives and therefore unaware whether they are satisfied by the consequences of behavior, is removed. It renders more readily understandable the learning of the manifold subtle attitudes, prejudices, mannerisms, and other responses of daily life

[27] "Mmm-hmm" evidently is a secondary reinforcing stimulus of a complex type as are many other symbols of social approval. It is interesting to speculate upon the nature of non-directive therapy (Rogers, 1942) in the light of Greenspoon's results.

[28] Certain experiments summarized by Thorndike (1935b) have aroused criticism and have stimulated repetition. Cf. Irwin, Kaufman, Prior, and Weaver (1934), Irwin (1935), Thorndike and Rock (1935), and Herrick (1939). Where experimenters have failed to find learning without awareness in particular situations, the fact stands, but does not demonstrate that such learning does not occur.

for which there is awareness neither of motivation nor of what is being learned. In like manner, it makes intelligible the acquisition of many behavior characteristics called abnormal, the motives to which are unreportable though readily inferable.

REFERENCES

Anderson, A. C. Time discrimination in the white rat. *J. comp. Psychol.*, 1932, 13, 27-55.

Bernard, J. A note on non-informative shock. *J. exp. Psychol.*, 1941, 29, 407-412.

Bernard, J. The specificity of the effect of shock on the acquisition and retention of motor and verbal habits. *J. exp. Psychol.*, 1942, 31, 69-78.

Brandt, H. The spread of the influence of reward to bonds remote in sequence and time. *Arch. Psychol., N. Y.*, 1935, 27, No. 180.

Brogden, W. J. The effect of frequency of reinforcement upon the level of conditioning. *J. exp. Psychol.*, 1939, 24, 419-431.

Brown, J. S. The generalization of approach responses as a function of stimulus intensity and strength of motivation. *J. comp. Psychol.*, 1942a, 33, 209-226.

Brown, J. S. Factors determining conflict reactions in difficult discriminations. *J. exp. Psychol.*, 1942b, 31, 272-292.

Brunswik, E. Probability as a determiner of rat behavior. *J. exp. Psychol.*, 1939, 25, 175-197.

Bugelski, R. Extinction with and without sub-goal reinforcement. *J. comp. Psychol.*, 1938, 26, 121-134.

Bunch, M. E. Certain effects of electric shock in learning a stylus maze. *J. comp. Psychol.*, 1935, 20, 211-242.

Carr, H. A. *Psychology: a study of mental activity.* New York: Longmans, Green, 1925.

Carr, H. A. Teaching and learning. *J. genet. Psychol.*, 1930, 37, 189-218.

Carr, H. A. The law of effect: a round table discussion. I. *Psychol. Rev.*, 1938, 45, 191-199.

Clements, F. E. The effect of time on distance discrimination in the albino rat. *J. comp. Psychol.*, 1928, 8, 317-324.

Cole, L. E. A comparison of the factors of practice and knowledge of experimental procedure in conditioning the eyelid response of human subjects. *J. gen. Psychol.*, 1939, 20, 349-373.

Cowles, J. T. Food tokens as incentives for learning by chimpanzees. *Comp. Psychol. Monogr.*, 1937, 14, No. 71.

Crespi, L. P. Quantitative variation of incentive and performance in the white rat. *Amer. J. Psychol.*, 1942, 55, 467-517.

Crespi, L. P. Amount of reinforcement and level of performance. *Psychol. Rev.*, 1944, 51, 341-357.

DeCamp, J. E. Relative distance as a factor in the white rat's selection of a path. *Psychobiol.*, 1920, 2, 245-253.

Dennis, W. Goal gradient or entrance gradient? *Psychol. Rev.*, 1935, 42, 117-121.

Denny, M. R. The role of secondary reinforcement in a partial reinforcement learning situation. *J. exp. Psychol.*, 1946, 36, 373-389.

Dollard, J., and Miller, N. E. *Personality and psychotherapy*. New York: McGraw-Hill, 1950.
Ehrenfreund, D. Effect of a secondary reinforcing agent in black-white discrimination. *J. comp. physiol. Psychol.*, 1949, 42, 1-5.
Eisenson, J. Confirmation and information in reward and punishment. *Arch. Psychol., N. Y.*, 1935, 27, No. 181.
Ellson, D. G. The acquisition of a token-reward habit in dogs. *J. comp. Psychol.*, 1937, 24, 505-522.
Estes, W. K. Generalization of secondary reinforcement from the primary drive. *J. comp. physiol. Psychol.*, 1949, 42, 286-295.
Farber, I. Spread of effect of reward and punishment in a multiple-choice situation. *Proc. Iowa Acad. Sci.*, 1941, 48, 313-317.
Finger, F. W. The effect of varying conditions of reinforcement upon a simple running response. *J. exp. Psychol.*, 1942a, 30, 53-68.
Finger, F. W. Retention and subsequent extinction of a simple running response following varying conditions of reinforcement. *J. exp. Psychol.*, 1942b, 31, 120-133.
Freud, S. *Introductory lectures on psychoanalysis*. London: Allen and Unwin, 1933.
Gantt, W. H. The nervous secretion of saliva: the relation of the conditioned reflex to the intensity of the unconditioned stimulus. *Proc. Amer. Physiol. Soc., Amer. J. Physiol.*, 1938, 123, 74 (Abstract).
Gilbert, R. W. The effect of non-informative shock upon maze learning and retention with human subjects. *J. exp. Psychol.*, 1936, 19, 456-466.
Gilbert, R. W. A further study of the effect of non-informative shock upon learning. *J. exp. Psychol.*, 1937, 20, 396-407.
Goodfellow, L. D. A psychological interpretation of the results of the Zenith radio experiments in telepathy. *J. exp. Psychol.*, 1938, 23, 601-632.
Grice, G. R. An experimental study of the gradient of reinforcement in maze learning. *J. exp. Psychol.*, 1942, 30, 475-489.
Grice, G. R. The relation of secondary reinforcement to delayed reward in visual discrimination learning. *J. exp. Psychol.*, 1948, 38, 1-16.
Grindley, G. C. Experiments on the influence of the amount of reward on learning in young chickens. *Brit. J. Psychol.*, 1929, 20, 173-180.
Guthrie, E. R. *The psychology of human conflict*. New York: Harpers, 1938.
Hamilton, E. L. The effect of delayed incentive on the hunger drive in the white rat. *Genet. Psychol. Monogr.*, 1929, 5, 131-207.
Heathers, G. L. The avoidance of repetition of a maze reaction in the rat as a function of the time interval between trials. *J. Psychol.*, 1940, 10, 359-380.
Herrick, C. J. Awareness in the inductive solution of problems using words as stimuli. *Arch. Psychol., N. Y.*, 1939, 33, No. 234.
Hilgard, E. R. *Theories of learning*. New York: Appleton-Century-Crofts, 1948.
Hull, C. L. Knowledge and purpose as habit mechanisms. *Psychol. Rev.*, 1930, 37, 511-525.
Hull, C. L. The goal gradient hypothesis and maze learning. *Psychol. Rev.*, 1932, 39, 25-43.
Hull, C. L. Special review of Thorndike, *The fundamentals of learning*. *Psycol. Bull.*, 1935, 32, 807-823.
Hull, C. L. Notes on the Humphreys extinction paradox. Unpubl. symposium notes, Midwestern Psychol. Assn., 1941.
Hull, C. L. *Principles of behavior*. New York: Appleton-Century, 1943.

Hull, C. L. Behavior postulates and corollaries—1949. *Psychol. Rev.*, 1950, 57, 173-180.

Humphreys, L. G. The effect of random alternation of reinforcement on the acquisition and extinction of conditioned eyelid reactions. *J. exp. Psychol.*, 1939a, 25, 141-158.

Humphreys, L. G. Acquisition and extinction of verbal expectations in a situation analogous to conditioning. *J. exp. Psychol.*, 1939b, 25, 294-301.

Humphreys, L. G. Extinction of conditioned psychogalvanic responses following two conditions of reinforcement. *J. exp. Psychol.*, 1940, 27, 71-75.

Humphreys, L. G. The strength of a Thorndikian response as a function of the number of practice trials. *J. comp. Psychol.*, 1943, 35, 101-110.

Irwin, F. W. A rejoinder. *J. exp. Psychol.*, 1935, 18, 389.

Irwin, F. W., Kaufman, K., Prior, G., and Weaver, H. B. On "learning without awareness of what is being learned." *J. exp. Psychol.*, 1934, 17, 823-827.

Jenkins, W. O. Studies in spread of effect. I. The bi-directional gradient in the performance of white rats in a linear maze. *J. Psychol.*, 1943a, 35, 41-56.

Jenkins, W. O. Studies in spread of effect. II. The effect of increased motivation on the bi-directional gradient. *J. comp. Psychol.*, 1943b, 35, 57-63.

Jenkins, W. O. Studies in spread of effect. III. The effect of increased incentive on the bi-directional gradient. *J. comp. Psychol.*, 1943c, 35, 65-72.

Jenkins, W. O., and Cunningham, L. M. The guessing-sequence hypothesis, the "spread of effect," and number-guessing habits. *J. exp. Psychol.*, 1949, 39, 158-168.

Jenkins, W. O., McFann, H., and Clayton, F. L. A methodological study of extinction following aperiodic and continuous reinforcement. *J. comp. physiol. Psychol.*, 1950, 43, 155-167.

Jenkins, W. O., and Rigby, M. K. Partial (periodic) versus continuous reinforcement in resistance to extinction. *J. comp. physiol. Psychol.*, 1950, 43, 30-40.

Jenkins, W. O., and Sheffield, F. D. Rehearsal and guessing habits as sources of the "spread of effect." *J. exp. Psychol.*, 1946, 36, 316-330.

Kaufman, E. L., and Miller, N. E. Effect of number of reinforcements on strength of approach in an approach-avoidance conflict. *J. comp. physiol. Psychol.*, 1949, 42, 65-74.

Kuo, Z. Y. The nature of unsuccessful acts and their order of elimination in animal learning. *J. comp. Psychol.*, 1922, 2, 1-27.

Lawrence, D. H., and Miller, N. E. A positive relationship between reinforcement and resistance to extinction produced by removing a source of confusion from a technique that had produced opposite results. *J. exp. Psychol.*, 1947, 37, 494-509.

Lorge, I. The efficacy of intensified reward and of intensified punishment. *J. exp. Psychol.*, 1933a, 16, 177-207.

Lorge, I. The effect of the initial chances for right responses upon the efficacy of intensified reward and of intensified punishment. *J. exp. Psychol.*, 1933b, 16, 362-373.

Lorge, I., Eisenson, J., and Epstein, B. Further experiments in the strength of connections where the connection is punished or rewarded or neither punished nor rewarded. *J. exp. Psychol.*, 1934, 17, 412-423.

Lorge, I., and Thorndike, E. L. The comparative strengthening of a connection by one or more occurrences of it in cases where the connection was punished and was neither punished nor rewarded. *J. exp. Psychol.*, 1933, 16, 374-382.

May, M. A. Experimentally acquired drives. *J. exp. Psychol.*, 1948, 38, 66-77.

Miller, N. E. Experimental studies of conflict. *In* Hunt, J. McV. (Ed.) *Personality and the behavior disorders.* Vol. I., 431-465. New York: Ronald Press, 1944.

Miller, N. E. Studies of fear as an acquirable drive: Fear as motivation and fear-reduction as reinforcement in the learning of new responses. *J. exp. Psychol.*, 1948, 38, 89-101.

Miller, N. E., and Dollard, J. *Social learning and imitation.* New Haven: Yale Univ. Press, 1941.

Miller, N. E., and Miles, W. R. Effect of caffeine on the running speed of hungry, satiated, and frustrated rats. *J. comp. Psychol.*, 1935, 20, 397-412.

Mowrer, O. H. A stimulus-response analysis of anxiety and its role as a reinforcing agent. *Psychol. Rev.*, 1939, 46, 553-565.

Mowrer, O. H. The law of effect and ego psychology. *Psychol. Rev.*, 1946, 53, 321-334.

Mowrer, O. H., and Jones, H. Habit strength as a function of the pattern of reinforcement. *J. exp. Psychol.*, 1945, 35, 293-310.

Muenzinger, K. F. Motivation in learning. I. Electric shock for correct response in the visual discrimination habit. *J. comp. Psychol.*, 1934, 17, 267-277.

Muenzinger, K. F. Reward and punishment. *Univ. Colo. Studies, General Series (A)*, 1946, 27, No. 4.

Muenzinger, K. F., Bernstone, A. H., and Richards, L. Motivation in learning. VIII. Equivalent amounts of electric shock for right and wrong responses in visual discrimination learning. *J. comp. Psychol.*, 1938, 26, 177-185.

Muenzinger, K. F., and Dove, C. C. Serial learning. I. Gradients of uniformity and variability produced by success and failure of single responses. *J. gen. Psychol.*, 1937, 16, 403-413.

Muenzinger, K. F., Dove, C. C., and Bernstone, A. H. Serial learning. II. The bi-directional goal gradient in the endless maze. *J. genet. Psychol.*, 1937, 50, 229-241.

Muenzinger, K. F., and Fletcher, F. M. Motivation in learning. VII. The effect of an enforced delay at the point of choice in the visual discrimination habit. *J. comp. Psychol.*, 1937, 23, 383-392.

Nuttin, J. Respective effectiveness of success and task-tension in learning. *Brit. J. Psychol.*, 1947a, 38, 49-55.

Nuttin, J. La loi de l'effet et la finalité du comportement. *Miscellanea Psychologica Albert Michotte*, 1947b, 611-633.

Pavlov, I. P. *Conditioned reflexes* (trans. by G. V. Anrep), London: Oxford Univ. Press, 1927.

Perin, C. T. A quantitative investigation of the delay-of-reinforcement gradient. *J. exp. Psychol.*, 1943a, 32, 37-51.

Perin, C. T. The effect of delayed reinforcement upon the differentiation of bar responses in white rats. *J. exp. Psychol.*, 1943b, 32, 95-109.

Perkins, C. C., Jr. The relation of secondary reward to gradients of reinforcement. *J. exp. Psychol.*, 1947, 37, 377-392.

Perrin, F. A. C. Conscious analysis versus habit hierarchies in the learning process. *J. comp. Psychol.*, 1921, 1, 287-308.

Postman, L. The history and present status of the law of effect. *Psychol. Bull.*, 1947, 44, 489-563.

Proshansky, H., and Murphy, G. The effects of reward and punishment on perception. *J. Psychol.*, 1942, 13, 295-305.

Rock, R. T., Jr. The influence upon learning of the quantitative variation of after-effects. Teach. Coll. Contr. Educ., 1935, No. 650.

Rogers, C. R. *Counseling and psychotherapy.* Boston: Houghton Mifflin, 1942.

Saltzman, I. J. Maze learning in the absence of primary reinforcements: A study of secondary reinforcement. *J. comp. physiol. Psychol.*, 1949, 42, 161-173.

Sams, C. F., and Tolman, E. C. Time discrimination in white rats. *J. comp. Psychol.*, 1925, 5, 255-263.

Seashore, H., and Bavelas, A. The functioning of knowledge of results in Thorndike's line-drawing experiment. *Psychol. Rev.*, 1941, 48, 155-164.

Sheffield, F. D. "Spread of effect" without reward or learning. *J. exp. Psychol.*, 1949, 39, 575-579.

Sheffield, F. D., and Roby, T. B. Reward value of a non-nutritive sweet taste. *J. comp. physiol. Psychol.*, 1950, 43, 471-481.

Sheffield, V. F. Extinction as a function of partial reinforcement and distribution of practice. *J. exp. Psychol.*, 1949, 39, 511-526.

Skinner, B. F. *The behavior of organisms: an experimental analysis.* New York: Appleton-Century, 1938.

Skinner, B. F. The processes involved in the repeated guessing of alternatives. *J. exp. Psychol.*, 1942, 30, 495-503.

Skinner, B. F. "Superstition" in the pigeon. *J. exp. Psychol.*, 1948, 38, 168-172.

Smith, M. H., Jr. Spread of effect is the spurious result of non-random response tendencies. *J. exp. Psychol.*, 1949, 39, 355-368.

Solomon, R. L. A note on the alternation of guesses. *J. exp. Psychol.*, 1949, 39, 322-326.

Solomon, R. L., and Wynne, L. C. Avoidance conditioning in normal dogs and in dogs deprived of normal autonomic functioning. *Amer. Psychologist*, 1950, 5, 264 (Abstract).

Spence, K. W. The role of secondary reinforcement in delayed reward learning. *Psychol. Rev.*, 1947, 54, 1-8.

Stephens, J. M. The influence of punishment on learning. *J. exp. Psychol.*, 1934a, 17, 536-555.

Stephens, J. M. Further notes on punishment and reward. *J. genet. Psychol.*, 1934b, 44, 464-472.

Stephens, J. M., and Baer, J. A. The influence of punishment on learning when the opportunity for inner repetition is reduced. *Ped. Sem. and J. genet. Psychol.*, 1937, 51, 209-217.

Stone, G. R. The effect of negative incentives in serial learning. III. Fixation due to an isolated verbal punishment. *J. gen. Psychol.*, 1948, 38, 207-216.

Stone, G. R. The effect of negative incentives in serial learning. II. Incentive intensity and response variability. *J. gen. Psychol.*, 1950, 42, 179-224.

Thompson, M. E. An experimental investigation of the gradient of reinforcement in maze learning. *J. exp. Psychol.*, 1944, 34, 390-403.

Thompson, M. E., and Dove, C. C. A further study on the bi-directional goal gradient in the endless maze. *J. exp. Psychol.*, 1943, 33, 447-456.

Thorndike, E. L. *Animal intelligence: experimental studies.* New York: Macmillan, 1911.

Thorndike, E. L. The law of effect. *Amer. J. Psychol.*, 1927, 39, 212-222.

Thorndike, E. L. *Human learning.* New York: Appleton-Century-Crofts, 1931.

Thorndike, E. L., *et al. The fundamentals of learning.* New York: Teachers College, Columbia Univ., 1932.

Thorndike, E. L. An experimental study of rewards. *Teach. Coll. Contr. Educ.*, 1933a, No. 580.

Thorndike, E. L. A theory of the action of the after-effects of a connection upon it. *Psychol. Rev.*, 1933b, 40, 434-439.

Thorndike, E. L. A proof of the law of effect. *Science*, 1933c, 77, 173-175.

Thorndike, E. L. The spread of the influence of reward to connections irrelevant to the learner's purposes. *J. genet. Psychol.*, 1934, 44, 428-435.

Thorndike, E. L. *The psychology of wants, interests, and attitudes.* New York: Appleton-Century, 1935.

Thorndike, E. L. The influence of occurrence, reward and punishment upon connections that had already considerable strength. *Miscellanea Psychologica Albert Michotte*, 1947, 308-322.

Thorndike, E. L., and Forlano, G. The influence of increase and decrease of the amount of reward upon the rate of learning. *J. educ. Psychol.*, 1933, 24, 401-411.

Thorndike, E. L., and Lorge, I. The influence of relevance and belonging. *J. exp. Psychol.*, 1935, 18, 574-584.

Thorndike, E. L., and Rock, R. T., Jr. Learning without awareness of what is being learned or intent to learn it. *J. exp. Psychol.*, 1934, 17, 1-19.

Tilton, J. W. The effect of "right" and "wrong" upon the learning of nonsense syllables in multiple choice arrangement. *J. educ. Psychol.*, 1939, 30, 95-115.

Tilton, J. W. Gradients of effect. *J. genet. Psychol.*, 1945, 66, 3-19.

Trowbridge, M. H., and Cason, H. An experimental study of Thorndike's theory of learning. *J. gen. Psychol.*, 1932, 7, 245-258.

Tuckman, J. The influence of varying amounts of punishment on mental connections. *Teach. Coll. Contr. Educ.*, 1933, No. 590.

Waits, J. V. The law of effect in the retained situation. *Arch. Psychol., N. Y.*, 1937, 30, No. 208.

Warden, C. J., and Haas, E. L. The effect of short intervals of delay in feeding upon speed of maze learning. *J. comp. Psychol.*, 1927, 7, 107-116.

Watson, J. B. The effect of delayed feeding upon learning. *Psychobiol.*, 1917, 1, 51-60.

White, A. E., and Tolman, E. C. A note on the elimination of short and long blind alleys. *J. comp. Psychol.*, 1923, 3, 327-331.

Wolfe, J. B. The effect of delayed reward upon learning in the white rat. *J. comp. Psychol.*, 1934, 17, 1-22.

Wolfe, J. B. Effectiveness of token-rewards for chimpanzees. *Comp. Psychol. Monogr.*, 1936, 12, No. 60.

Wolfe, J. B., and Kaplon, M. Effect of amount of reward and consummative activity on learning in chickens. *J. comp. Psychol.*, 1941, 31, 353-361.

Yoshioka, J. G. Weber's law in the discrimination of maze distance by the white rat. *Univ. Calif. Publ. Psychol.*, 1929, 4, 155-184.

Zeaman, D. Response latency as a function of the amount of reinforcement. *J. exp. Psychol.*, 1949, 39, 466-483.

Zirkle, G. A. Success and failure in serial learning. I. The Thorndike effect. *J. exp. Psychol.*, 1946a, 36, 230-236.

Zirkle, G. A. Success and failure in serial learning. II. Isolation and the Thorndike effect. *J. exp. Psychol.*, 1946b, 36, 302-315.

VIII

THE ROLE OF FREQUENCY

INTRODUCTION

ONE of the few psychological principles which is ancient and revered enough to have become embedded in popular speech is the maxim that "practice makes perfect." Like many another popular principle, this one is, at best, a half truth. It fails to specify the type of practice, the amount of practice, and the degree of perfection. It is apparent, from all that has been said in the preceding chapters, that there are different kinds of practice and that these are not all equally effective in producing improvements in performance. However, it is true, over a wide range of conditions, that performance does improve with increasing practice or frequency of repetition. Frequency, in this context, refers to the number of trials or repetitions of learned act, and it is generally the variable against which improvement is plotted when learning curves are drawn. The word, frequency, necessarily implies a frequency of *something*—a specifiable recurrence of an identifiable set of events. In the case of some learning materials, frequency is easily defined, as, for example, the number of repetitions of a list of nonsense syllables or the number of reinforced conditioning trials. On the other hand, in the case of some activities, such as pursuit-rotor learning, the division of practice into segments or trials is more or less arbitrary. In this chapter, we shall examine some of the kinds of events which can occur together with greater or lesser frequency, and we shall attempt to show, in each case, the relationship between such frequency and performance.

Frequency of a Stimulating Situation

The first group of experiments to be described is one in which a stimulating situation is presented frequently with no attempt on the

part of the experimenter to control the response of the subject. The situation is one to which two or more different responses may be made. What influence will continued repetitions of this situation, with no knowledge of results or other effect given to the subject, have on the frequency of response? Concretely, let a three-choice situation be presented ten times and let the three possible responses or choices be elicited with frequencies of 6, 3, and 1. Will continued presentation of the situation tend to change the proportions of the three responses in some progressive way, or will it tend to leave them unaltered?

Thorndike (1931, 1932) has devoted an extensive series of experiments to this question, giving the subject in each case no formal information concerning the adequacy of his responses. A few of the experiments will be briefly described. The number of records is high and need not be given. (a) The subject was presented with strips of paper varying in length by steps of a fourth of an inch from 5 inches to 11 inches and was asked to estimate the length of each. (b) The subject, sitting with closed eyes, was presented with the instruction to draw a 2-inch line (or a 4-inch, 6-inch, or 8-inch line) with one quick shove of the pencil. (c) The experimenter read to the subject a long series of auditory stimuli at a constant rate, the subject being instructed to respond to each by writing any number between 1 and 9, or some one of other limited ranges of response. Sometimes certain situations were presented more frequently than others, sometimes not. In all, the relative frequencies of responses in the first 10 (or other number) could be compared with the relative frequencies in later 10's and particularly in the last 10.

The 28 experiments on this problem show that with repetition of a stimulating situation initially frequent responses do not gain at the expense of the initially less frequent. Frequency of a situation does not lead to learning when the responses to the situation are not followed by differential consequences. For that matter, why should an instruction, no matter how often given, lead to the selection of a particular response thereto, if none of the responses is followed by a consequence which informs the subject whether the instruction had been successfully carried out? The experiments

show that no such selection occurs on the basis of frequency of the situation alone.[1]

Changes did occur in the subject's responses, although not such as would have been predicted on an hypothesis that frequency of a situation will lead to a further strengthening of the initially most frequent responses to it. With continued presentation of the stimulating situation, there was a tendency for the subjects' responses to become stereotyped, but not in a way a frequency hypothesis would have predicted. There was a selection of response, presumably on the basis of self-instruction or of some informally induced set. Thus, subjects, presented with the first two or three letters of a word and asked to give a word beginning with those letters, tended to give an increasing number of short words where longer ones could have been given, as "elf" instead of "elephant" in response to the letters "el." The increasing stereotypy of response may readily have been a function of motive and effect. The short words were easier to write and permitted the subject to finish the task quickly in the presence of others. Behavioral changes may still be instances of learning, although they are not in the direction of the experimenter's criterion. Thus, in Thorndike's line-drawing experiment, some subjects approached more and more closely to some other length than the one they had been instructed to draw, as to a line 5.01 inches long instead of to one 4 inches long (cf. H. Seashore and Bavelas, 1941). The subject has his own standard of a 4-inch line and inevitably obtains some knowledge of results from the proprioceptive stimulation of drawing. Any progressive change may be interpreted as a function both of this knowledge of results and of possible changes in his standard. The subject's response approaches the standard of correctness he has adopted. This standard and his estimation of how his responses relate to it are the important conditions, not the frequency of the situation.

[1] Although it is possible to interpret one or more of the experiments reported by Thorndike (1931, 1932) on this problem and others in different ways from those he has chosen, and although difficulties may be found with certain of the experiments, there are other experiments which are free from the difficulties and which support his interpretations. The net result is support of his conclusions. A summary of the experiments and an evaluation will be found in McGeoch (1933) and Hull (1935).

Frequency of Situation and Response

In the experiments just cited, the stimulating situation is repeated by the experimenter, but the response is left to the subject, as in the case when one asks, "How are you this morning?" the answer is undetermined by the asker. Another experimental approach is to present the subject with a series of paired terms—that is, both situation and response terms are presented, not situation alone—but without instruction to learn and without formal knowledge of results, e.g., when one casually notices the state and license number of a passing car. The paired terms may be given any desired frequency, and the experimental problem is to discover if the frequency of the pairing fixates.

The control conditions in experiments on set and learning, and the experiments on incidental learning are of this kind. We have seen that with a set only to observe, but not to learn, learning is very slow, even when frequency is high, and that at least some of the learning which does occur is a function of informal sets. It is a reasonable hypothesis that all may be.

In a study of the influence of frequency of situation and response, Thorndike (1931, 1932) has used a number of different experimental arrangements to insure that the paired terms are temporally contiguous and that response to them is free from the influence of effect. The result is that a repeated sequence of items, with the hearer instructed to listen comfortably as he might to a lecture, produces little or no learning.

One experiment leading to this conclusion employed a long list of word-number pairs, with certain pairs occurring relatively often and in certain sequences. In a series of 1304 pairs, four of them (*dregs* *91*, *charade 17*, *swing* 62, and *antelope* 35) appeared frequently and always so that *dregs* came just after 42, *charade* after 86, swing after 94, and *antelope* after 97. After hearing the list, the subjects were asked what numbers came just after certain words (*dregs*—what number?) and which words came just after certain numbers (42—what word?). The percentage of correct numbers given in response to the word member of the pair, when the paired word

and number had been repeated 19 or 21 times scattered through the whole series, was 37.5. The figure for recall of words, which as the first member of a pair had followed the number of the preceding pair 24 times during the series, was half of 1 per cent, an amount which could have been reached by guessing.

The insignificant learning of the words which came next after the last member (number) of the preceding pair demonstrates the slight potency of combined contiguity and frequency, while the much greater learning of the second member of a pair in response to the first member of that pair illustrates the concept of *belonging*, which Thorndike has introduced.

The extinction of conditioned responses provides another instance of repetition of a stimulating situation and a response to it, but one in which the frequency correlates positively with elimination of the response. In the usual conditioning experiment the conditioned stimulus is followed by the unconditioned stimulus, which is said to *reinforce* the conditioned response, and this action of reinforcement is basic in all conditioning (Hull, 1934; Hilgard and Marquis, 1940). The food must follow the bell, the shock must follow the buzzer, the puff of air to the eyeball must follow the light shone in the eye. If, after a conditioned response has been established, the conditioned stimulus is frequently presented without the reinforcing unconditioned stimulus, the response will continue to appear at first, but quickly shows a decrement which proceeds until the conditioned response ceases to be elicited, or is extinguished. A decrement apparently analogous to extinction has been found by Peak and Deese (1937) in verbal learning.

The chief condition of extinction is the withdrawal of reinforcement. During the extinction trials, the stimulating situation is frequent, and with it at first comes a repetition of the response, so that there is frequency of situation and response, but frequency without reinforcement. The result is at least a temporary elimination of the response. That it may later exhibit spontaneous recovery does not alter the fact that during extinction frequency without reinforcement has led to elimination.

Frequency of Situation and Response with Belonging

In the word-number pairs of the experiment by Thorndike on frequency of a stimulating situation and response, the word and number of a pair *belong* together in a way that the number of one pair and the word of the next do not. In like manner, the words of a sentence belong together in a way that the last word of one and the first word of the next do not; and within a sentence certain words belong together as other words do not. Subject and predicate, verb and adverb, adjective and noun, belong together far more than adjective and verb or adjective and adverb. When belongingness is added to repetition in Thorndike's experiments, learning occurs, but it is slow. Where frequency alone is minimally effective or not at all, frequency plus belonging does, however, yield some fixation.

Effect was kept at a minimum or at zero, as in an experiment where subjects copied long lists of numbers such as *218 97*, some of which occurred more frequently than others, and were then tested by being asked to give the two-figure number in response to the three-figure number. The subjects thought that the experiment was on fatigue, not learning. Frequency with belonging and minimal satisfying aftereffect led to some fixation.

Sentences like those cited below were read ten times to subjects who were instructed to listen with moderate attention as they would to a lecture.

Alfred Dukes and his sister worked sadly. Edward Davis and his brother argued rarely. Francis Bragg and his cousin played hard, Barney Croft and his father watched earnestly. Lincoln Blake and his uncle listened gladly.

Questions which tested association from the end of one sentence to the beginning of the next showed practically no learning. When tested in the forward direction, first and last names of persons showed a considerable amount of learning, while the subject-predicate terms showed still more. Other experiments permit the conclusion that learning tends to increase with degree of belong-

ingness, being very slight when belongingness is low and rising to a relatively high point when belongingness is very high.

Strictly speaking, belongingness is belonging *for* an individual, but when one has reason to be sure that a given material has belongingness for the sampling of individuals who learn it, one may speak of the belonging as a characteristic of the material without danger of misunderstanding. Most psychologists would probably agree that the illustrations given represent an important and discriminable characteristic of the materials. The last word of one sentence does not "go with" the first word of the next as much as with the words before that last word, and so on. But on the nature of the general concept *belongingness* there is no agreement. Thorndike's experiments show that the variable he calls belonging does influence learning, where frequency without it does not; but what is the nature of belonging?

Thorndike (1932, p. 72) says that it may be no more than a recognition that "this goes with that." It need have "nothing logical, or essential, or inherent, or unifying in it." It seems not to be meaning, inasmuch as apparently meaningful pairs like *last* 99 and *foot 12* are learned little, if at all, better than apparently neutral pairs.

It is a reasonable hypothesis that belonging is a function of transfer in terms of general factors, particularly a set to note and react to certain kinds of events, or a general habit of regarding such events as going together. Consider the amount of training any human being, even a young one, has had in the structure of his own language. Subjects go with predicates, adverbs with verbs, last names with first ones. Sentences are closed units and lead on from each one to the next as from unit to unit, not as from one word to another.

General habit and set cooperate closely and may often amount to the same thing in the context of both language and non-language responses. Human beings are habituated to taking certain kinds of instructions and sets from others and from the environment, to considering certain things as important and acceptable and relevant, and to neglecting other things and relations. They are often habituated to noting and reacting to the recurrence of items in a series

which contains little repetition, and so on.[2] Many, if not all, of the instances of belonging in Thorndike's experiments and in everyday life may be regarded as being a function of transfer.

Why, on this view, does belonging lead to learning? In the same way that any positive transfer does; in the case of belonging, by causing the subject to react to certain kinds of relations and to fit them into already habitual patterns of response, which act as a mnemonic system. Degree of belongingness is then a function of the extent to which such transfer occurs, and ease of learning should be roughly in proportion to degree of belongingness. Associative generalization, as well as nonspecific transfer, may sometimes be involved.

A minimum of satisfying consequence following a response may be sufficient for fixation when the transfer described is present. This minimal consequence or effect might be provided by the sheer operation of an already established habit or of a transferred set. The performance of well-formed modes of acting can in itself become a motivating condition, and the carrying out of the performance a satisfying consequence. The reading of a first and last name together, or a subject and a predicate, or both, has a certain belongingness and is more satisfying, right, and adequate than is reading the last word of one sentence and the first word of the next in sequence.

An alternative interpretation is that the transferred factors assimilate the new items without the influence of effect, that the habit of reacting to first and last names together is a general one to which a new first and last name are assimilated at once and fixated. On this view, transfer or associative generalization might be regarded

[2] This may account for the recall of repeated numbers from a list of 100 two-digit numbers read at the rate of one every two seconds, the subjects being instructed only to listen (Pratt, 1936). That is, the subjects may have thought, "There's the same number again," and doing this may have fixated some of the numbers which recurred between one and eleven times. The introduction of a set to discover whether one number was read oftener than another increases the recall of the repeated items, while a set to note all numbers containing zeros reduced the recall of recurrent items. The results may be interpreted as being a function of frequency of operation of a set, a transferred set in the case of no special instructions.

(a) as a fundamental condition of fixation, secondary to effect because what transfers must have been learned as a function of effect in the first instance, but otherwise coordinate, or (b) as the appearance in a new situation of acts which had already been learned in similar situations in the past.

The response to first and last names as belonging together is easy to understand as a function of an already formed habit, but how does this habit fixate these particular names under either (a) or (b)? Since this cannot be answered at present, and since the operation of transfer plus minimal aftereffect will account for belonging with little difficulty, this seems the more acceptable view.

Professor Thorndike might have said that this interpretation does not fit his interpretation of belongingness. Certainly, it hypothesizes the action of aftereffects to cooperate with belonging, which his interpretation does not. There are other possible interpretations. The one tentatively suggested here seems to the writers the most reasonable way of accounting for the influence of belonging, a way which has the advantage of explaining the phenomenon in terms of other and better-known facts.

But why does belonging lead to no faster learning than it does in Thorndike's experiments? Many of the experiments contained large opportunities for intraserial interference, so that learning which had occurred could easily have been inhibited by items intervening between it and the next presentation of the first learned ones (Thorndike, 1931, p. 27). Most of the series were long and the opportunities for interference correspondingly great. Since there was no formal instruction to learn the items, and since the rate of presentation was usually rapid, the transferred general factors must have operated casually and briefly. This, in conjunction with interference, would account for the slow learning.

Whatever belonging may be interpreted to mean, Thorndike's experiments prove that frequency of certain kinds of terms is a positive determiner of fixation—that is, frequency of terms that seem to the experimenter, and probably to the subject, to go together or to belong together. If, as seems reasonable, we interpret belonging to be a function of transfer of general factors and of associative gen-

eralization plus at least minimal aftereffect, then the experiments show that frequency of materials to which there is transfer of general factors promotes fixation.

Guided Frequency of Correct Responses

The role of frequency in learning a perceptual-motor problem may be studied by guiding the subject through the correct responses a given number of times. This amounts to forcing the correct series of acts, or to giving forced frequency, in a situation which otherwise would be attacked by trial and error. With guidance the learner does not discover the solution independently, errors are prevented or minimized, and nothing seems left for the subject to do but to fixate the activity through which he is guided under instruction to try to profit from the guidance. Guidance in a trial-and-error problem thus provides forced frequency of the correct acts and sharply limits trial and error, often reducing it to zero, but motivation is probably less and the influence of its satisfaction smaller than when the subject is practicing independently.

A large part of our information on guidance has come from a series of investigations by Carr and his students in which the chief instrument has been the stylus maze. The work up to 1930 has been summarized and interpreted by Carr (1930). The typical procedure is to introduce guidance for a certain number of trials at any desired region of practice and then to discontinue it, leaving the subject to practice by himself until the criterion is reached. The major modes of guidance will be summarized, and one typical investigation of each mode listed. (a) *Mechanical guidance* was accomplished in the maze by blocking the entrances to the cul-de-sacs so that, for any desired number of runs, the subject could traverse the true path only (Koch, 1923). (b) In *manual guidance* the experimenter grasped the stylus below the point at which it was held by the subject and guided the subject's hand steadily and at a uniform rate over the true path from start to goal (Ludgate, 1923). (c) In one form of *verbal guidance* the experimenter told the subject what moves to make while traversing the maze, telling him to go straight ahead,

now to the right, and so on. In another form a signal was given when the subject started to make an error or different amounts of information were given about an error as soon as it was made (Wang, 1925). In still another the subjects were given varying amounts of information about the maze before practice began (Lambert and Ewert, 1932). Verbal guidance has also been applied to the solution of rational problems, the subjects being given varying kinds of information designed to aid solution (Waters, 1928). (d) *Visual guidance* has been given in a special type of maze, designed by Carr for this purpose, in which the true path and the cul-de-sacs cannot be distinguished from each other by means of vision. All paths look as if they lead to the goal, but all save the true path are blocked at the end by invisible stops (Carr, 1921). Under another condition the subject could see his moving hand as it held the stylus, but could not see the maze. In still another the subject was allowed to inspect an ordinary maze before each trial (Peterson and Allison, 1930). (e) In *graphical guidance*, another kind of visual guidance, the maze was behind a screen as in the usual maze experiment, and a diagram was given to the subject with instructions to utilize it. The influence of diagrams of varying degrees of completeness was also studied (Carr, 1921).[3]

These modes of guidance do not have equal significance for the problem of the role of frequency in learning. It is probable that they elicit different degrees of motivation and that they differ in the opportunity they give for transfer and effect to enter. All of them, however, demonstrate the influence of limitation of incorrect responses, and some of them, particularly mechanical and manual guidance, are of high importance for a knowledge of the influence of forced frequency of correct responses when minimal transfer and effect are present.

The effectiveness of guidance is a function of its amount and of the point in practice at which it is introduced. Small numbers (as 2 or 4) of guided trials early in practice (usually in the first few

[3] For more recent investigations using modified methods, but with results which are in the main continuous with the earlier ones, cf. Rubin-Rabson (1937), Cook (1939), and Sartain (1940).

trials and not beyond the tenth are the most effective in facilitating learning. The influence of guidance diminishes as its amount increases and as the point of its introduction is moved away from the early stages of practice. Large amounts of guidance, or amounts given at unfavorable points during practice, may hinder learning.

The influence of large amounts of correct frequency is demonstrated in an experiment by Waters (1930) in which different groups of college students were given 20, 40, and 80 manually guided trials in a maze.[4] If the guided trials are regarded as a prior condition, and if only the records subsequent to the guidance are considered, trial, error, and time records decrease as amount of guidance increases, except for the trials after 20 guided runs (Table XV), and

TABLE XV

MAZE RECORDS WITH VARYING AMOUNTS OF GUIDANCE

(From Waters, *J. gen. Psychol.*, 1930, 4, pp. 215 and 218)

Condition	Mean Scores Subsequent to Guidance			Mean Scores of Guided Plus Unguided Trials	
	Trials	Errors	Time (Sec.)	Trials	Time (Sec.)
Control	37.5	466.2	2191.0	37.5	2191.0
20 guided trials	42.8	314.5	1714.6	62.8	2114.6
40 guided trials	27.7	235.9	1433.8	67.7	2233.8
80 guided trials	23.4	192.8	1232.6	103.4	2832.6

it must be concluded that guidance has led to learning. If, however, the guided trials are considered as a component part of the total learning, as they legitimately are, trials increase as guidance increases, and time with 80 guided trials is considerably larger than for the control.[5]

From the standpoint of frequency, the table shows that, when

[4] The subjects were instructed to allow hand and arm to be as passive as possible, but to learn as much as possible about the maze in order to be able to traverse it unaided after withdrawal of guidance. Each guided run took twenty seconds, and rest intervals were introduced at regular points. The subject was guided down the center of the pathway, making as few contacts with the walls as possible.

[5] Errors are the same in both cases, since no errors occur in the guided runs.

20, 40, or 80 guided trials are given, a guided frequency of one is not equivalent to an unguided frequency of one. If it were, the groups given 40 and 80 guided runs should have learned by guidance alone. Guidance did, however, decrease the number of errors, although it did not decrease total time. Forced correct frequency in a stylus maze is the analogue of frequency of situation and response with belonging in the case of verbal items, where Thorndike has found that such frequency yields slow learning.

Guidance in a stylus maze does more than provide correct frequency alone. It facilitates orientation toward the goal, makes the significant aspects of the problems more identifiable, limits exploration, decreasing time and errors as a result, and particularly in some of its verbal forms gives knowledge of results when responses are wrong as well as when they are right. It seems also to be more effective the more it promotes and favors a positive response of the subject to the significant feature of the maze.[6] This is particularly true in a comparison of manual guidance versus correct verbal guidance over the true path (Wang, 1925), where the verbal instructions are accompanied by fewer errors.

The immediately important fact, however, is that in learning a problem in which trial and error is the characteristic mode of solution it makes a difference whether the frequency is applied to the guided correct responses only or to a solution discovered in the process of free trial and error. This implies that a complete process of learning includes more than the fixation of correct responses only, although this fixation is necessary if the criterion is to be attained. Elimination is bound up with fixation. In the practice of a complex act, at least, one learns what to do by making errors and correcting them, as well as by making the correct responses themselves.

[6] Cf. an experiment by Waters (1931) in which each subject in the experimental group was guided over the path taken by a control subject, including cul-de-sac entrances and retracings, having been informed in advance that this would be done. This form of guidance reduced the trials and cul-de-sac errors and increased retracting errors, total time, and final speed. It seems to have aided orientation and identifiability and to have induced a slower and more cautious mode of attack.

Frequency of Adequate and Inadequate Responses

As we have seen in Chapter VII, the consequences of an activity are powerful determiners of the learning of that activity. Whether or not the law of effect is considered to be a basic principle of learning, at an empirical level it is apparent that reinforcement, nonreinforcement, and punishment have an effect upon the learning process. The repetition of reinforced responses tends, on the whole, to strengthen those responses, and we may consider the frequency, in this case, to act as the bearer of effect (cf. Melton, 1941). The evidence which supports a reinforcement interpretation of learning has already been cited and need not be repeated here.

On the other hand, repetition of inadequate responses, either separately or in the context of some larger performance, may have an influence upon learning. The studies of guidance have demonstrated that prevention of error beyond a certain point may retard learning and that learning is relatively slow with forced repetition of the true path, only. The subject eventually learns the true path by entering the blind alleys and by retracing on the true path; each transit from start to goal may be by a different total path; and both right and wrong acts require repetition. The wrong acts are eliminated, in part, by being performed—that is, by frequency (Carr, 1930), in the sense that their elimination is positively correlated with their frequency.

Maze experiments, however, are complicated by the inevitable interspersing of correct and incorrect acts, so that the role of frequency of the wrong acts is difficult to isolate. The studies which have followed Dunlap's (1928) statement of the hypotheses that repetition may decrease the probability of later response or that it may have no influence at all upon fixation have concentrated for the most part on isolable responses. In support of the *beta hypothesis* [7] that frequency itself is powerless but may let negative factors

[7] This hypothesis was originally stated as one of three, called at that time the alpha, beta, and gamma postulates. In brief, the *alpha hypothesis* is that frequency of stimulating situation and response increases the probability that recurrence of the same or a similar situation will be followed by the same or a similar response. This is the old and familiar assumption that frequency fixates.

operate to eliminate the repeated response, Dunlap cited a number of informal observations. His own often-made error of writing *hte* for *the*, when typing rapidly, was eliminated by typing a half-page of *hte's* while holding the "futuric thought" that this was an error he would not make again. He also reported from work with students and patients that bad habits, such as thumb-sucking, nail-biting, and certain speech defects, had been eliminated by repetition with the intent to eliminate.

It will be noticed that in testing the hypothesis the subjects repeated the wrong or inadequate response with a set to eliminate it and to fixate another response not overtly practiced. The extent to which effect was present during this process is unknown, but there is reason to suppose that some effect was present. Experiments on the repetition of the act to be eliminated give frequency to a situation and an inadequate response and to the thought that this is an error which should be eliminated. Practice of the wrong response has often been called *negative practice*, though it is negative only in the sense that it is aimed at the inhibition of an act. Such inhibition as a function of practice is as positive a process as any in learning.

The operation of negative practice on an experimental scale is illustrated in an experiment by Holsopple and Vanouse (1929) with students of typing who had just begun to transcribe from shorthand notes. Eleven were found who had been making at least four automatic and apparently habitual errors in the spelling of words which, outside of transcription, they knew how to spell correctly. Each was told of his constant misspelling and was instructed to type, spelling as he had constantly misspelled them, eight full lines of each of two of the words. The other two constantly misspelled words, used as controls, were given an equal amount of

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The *beta hypothesis* is that frequency has no influence on fixation; and the *gamma hypothesis*, that frequency has a negative effect. As Peak (1941) has pointed out, the names "beta" and "gamma" have sometimes been interchanged. The view that frequency is ineffective in perception, or in what may be interpreted as response, which is a function of transfer, had been expressed before Dunlap (Gottschaldt, 1926) and has often been stated by the gestaltists.

correct practice—that is, with correct spelling. The result of the same frequency applied to both forms of practice was tested by dictation in which each of the four words appeared at least four times. On this test no student made an error in spelling a word which he had practiced incorrectly, while ten of the eleven students made errors on the words practiced correctly.[8]

More recently, Peak, Brooks, and Hobson (1941), using two well-equated groups of subjects, had one group practice ten words, previously misspelled twice on a spelling test, by writing them either sixteen or thirty-two times correctly, while the other group wrote the same number of words with the incorrect spelling. Tests after three days and three weeks showed that incorrect and correct practice had been equivalent in fixating the correct spellings. After three weeks the number of errors had increased, but there was still a retention of from 70 to 80 per cent of the corrections.

Negative practice has also been used by Kellogg and White (1935) with a stylus maze, in which the individual responses are less readily isolable from the total act. One group was forced to repeat each cul-de-sac error as soon as it was made, and a second was instructed to trace an equivalent section of true path opposite the error just made. Each group was compared with one which learned by the customary trial and error. If anything, the group which repeated each cul-de-sac error learned more rapidly than the two other groups, and in any case learned as rapidly as they did.

Negative practice has been found to be effective also in correcting errors in piano-playing and in typing, as well as in spelling. It may be concluded that frequency of situation and of inadequate (wrong) response to it may eliminate the wrong response. It does this when the association between situation and response is relatively isolable, as in the case of spelling errors, and when it is part of a larger problem, such as the maze. Dunlap (1932) believes that the results of negative practice are a special case of the fact that the

[8] A critique of the methods used in this and other experiments on negative practice has been published by Peak, Brooks, and Hobson (1941). This paper and one by Peak (1941) contain references to the work on the problem of negative practice.

acts one practices are not the acts one eventually learns and that the determining conditions are the "thoughts and desires" present during practice. He has not elaborated the modes of action of thoughts and desires whereby the practice of incorrect responses leads to the fixation of correct ones. It is not difficult to see a continuity between the effectiveness of making errors in maze learning (cf. the experiments on guidance) and of practicing errors by the method of negative practice.

In negative practice the subject is faced with a situation having at least two response alternatives, one right and the other (or others) wrong. Both are already in his repertoire of response, so that learning to make the right one consistently means fixating a correct choice between responses he can already make: *the* instead of *hte*, or a turn to the right instead of to the left. Repetition of the incorrect choice, under the conditions of negative practice, does certain things which may partly account for its effectiveness. It more clearly *identifies* the correct choice as the one to be made in that situation and the incorrect choice as something to be avoided. Prior to negative practice the subject *could* make the correct response, could identify it as the one to be made, but often did not. The practice fixates the making of the identification, and the subject's motivation to act correctly will force the choice of the correct response. The thoughts and desires which Dunlap emphasizes are intimately related to this action of identification and motivation.

Certain conditions may facilitate identification and correct choice. Negative practice may strengthen the set to identify the alternatives. It may do this by presenting the situation in a wider meaningful context. The subject undoubtedly thinks that the practice of a wrong spelling is a peculiar thing to do, and negative practice is likely to arouse a context of events which, by its richness and complexity of connection, will make more probable the identification of the correct choice when the word reappears in actual use. When the stimulating situation recurs, the subject may think, "This is the word I had to misspell," whereupon his set to spell it correctly elicits the right response. There is also the possibility that during negative practice

the subject is implicitly repeating the correct spelling with intent to learn it and getting positive consequences or effect by so doing.[9]

SUMMARY AND INTERPRETATION

The conclusions of the work on the role of frequency may be summarized briefly.

(a) When a stimulating situation is frequently presented with no specification of the response and no formal knowledge of results or effect, behavior does not change in the direction of the experimenter's criterion, although it may change toward some standard adopted by the subject. Responses which are frequent early in practice do not gain as a result of their initial frequency.

(b) If frequent presentation of contiguous terms (situation and response), without consequences specific to the connection, leads to learning, the learning is very slow. Furthermore, in the case of conditioning, when the situation-response connection fails to be followed by the accustomed reinforcement, the response diminishes. Thus, in the absence of consequences or reinforcement, there is little learning and withdrawal of the reinforcement tends to cause the dropping out of the response.

(c) When, however, the members of a situation-response pair belong together in some way, consequences being indeterminate but probably not great, learning occurs and increases as belongingness increases. An interpretation of belongingness as a case of transfer of training has been offered.

(d) Correct frequency of situation and response, forced by guidance, if given in small amounts and early in practice, facilitates

[9] A systematic study of the conditions under which negative practice is effective has yet to be made. The conditions listed state some of the probable conditions. For other interpretations of the results, many of which have been incorporated in the discussion in the text, cf. Guthrie (1935), Kellogg and White (1935), and Peak (1941). Peak's paper sets the results of negative practice in a wider context of learning theory. Some of the conditions of negative practice seem to have been operating in Peterson's (1922) experiment with a mental maze and with frequency on the side of the errors. The slow learners were slow to grasp the fact that an error returned them to the starting choice, but when this had been grasped and identification of what was being done had occurred, learning accelerated. Here, as elsewhere, the selective influence of set overweighed frequency.

learning. As amount of guidance increases, however, a point is soon reached at which a guided trial ceases to be as effective as an unguided one. Large amounts of correct and guided frequency may retard learning.

(e) Frequency of reinforced or rewarded situation-response connections is productive of large amounts of improvement, other factors being equal.

(f) The incorrect frequency of the so-called negative practice experiments eliminates the response repeated and fixates the correct, and not overtly repeated, response.

The conclusion that under many conditions frequency of stimulating situation, or of a situation and response, is accompanied by little or by very slow learning, and that frequency sometimes eliminates, may seem to contradict the progressive curves which curves of learning show, since one thing common to all practice is the frequency of repetition. This contradiction, however, is apparent rather than real. The term, frequency, means little unless one specifies what is frequent. Usually we are referring to a cluster of what we consider to be the conditions of learning. If some events other than the conditions of learning are frequent, it is not surprising that learning doesn't occur. Just what the conditions necessary for learning may be is a subject of considerable theoretical dispute.[10] That frequency is attended by different results under different conditions should cause no surprise, since the foregoing chapters have repeatedly shown that it does. Any condition which determines the amount of learning during the course of a single trial may be assumed to affect the course of learning over a number of trials.[11]

[10] A discussion of this, and of other theoretical problems concerning the law of frequency, may be found in Chapter II.

[11] There are possible exceptions to this statement. Guthrie (1935) believes that learned responses could be established in a single trial if all the conditions of subsequent stimulation were the same as those on the first presentation, since he holds that associations are established on an all-or-none basis as a function of contiguity of stimulus and response. A frequency of more than one is held to be necessary because with successive repetitions more and more stimulating cues come to be attached to the response, until finally enough have become connected with it to ensure its elicitation in most situations. Gestaltists who stress the importance of insightful solution hold that the optimal learning problem is one which permits the subject to survey the situation, to perceive

These effects may be expected to reveal themselves in the rate of improvement and in the final level of perfection which is attained.[12]

REFERENCES

Carr, H. A. The influence of visual guidance in maze learning. *J. exp. Psychol.*, 1921, 4, 399-417.

Carr, H. A. Teaching and learning. *J. genet. Psychol.*, 1930, 37, 189-218.

Cook, T. W. Guidance and transfer in part and whole learning of the disc transfer problem. *J. educ. Psychol.*, 1939, 30, 303-308.

Dunlap, K. *Habits: their making and unmaking.* New York: Liveright, 1932.

Gottschaldt, K. Über den Einfluss der Erfahrung auf die Wahrnehmung von Figuren. I. Über den Einfluss gehäufter Einprägung von Figuren auf ihre Sichtbarkeit in umfassenden Konfigurationen. *Psychol. Forsch.*, 1926, 8, 261-317.

Guthrie, E. R. *The psychology of learning.* New York: Harper, 1935.

Hilgard, E. R., and Marquis, D. G. *Conditioning and learning.* New York: Appleton-Century, 1940.

Holsopple, J. Q., and Vanouse, I. A note on the beta hypothesis of learning. *Sch. and Soc.*, 1929, 29, 15-16.

Hull, C. L. Learning. II. The factor of the conditioned reflex. *In* Murchison, C. (Ed.), *A Handbook of General Experimental Psychology.* Worcester: Clark Univ. Press, 1934, pp. 382-455.

Hull, C. L. Special review of Thorndike, *The fundamentals of learning. Psychol. Bull.*, 1935, 32, 807-823.

Kellogg, W. N., and White, E. R. A maze test of Dunlap's theory of learning. *J. comp. Psychol.*, 1935, 19, 119-148.

Koch, H. L. The influence of mechanical guidance upon maze learning. *Psychol. Monogr.*, 1923, 32, No. 147.

Lambert, J. F., and Ewert, P. H. Part I: The effect of verbal instruction upon stylus maze learning. *J. gen. Psychol.*, 1932, 6, 377-397.

Ludgate, K. E. The effect of manual guidance upon maze learning. *Psychol. Monogr.*, 1923, 33, No. 148.

McGeoch, J. A. Review of Thorndike, *The fundamentals of learning. J. gen. Psychol.*, 1933, 8, 285-296.

Melton, A. W. Article on learning. *In* Monroe, W. S. (Ed.), *Encyclopedia of educational research.* New York: Macmillan, 1941, pp. 667-686.

Peak, H. Negative practice and theories of learning. *Psychol. Rev.*, 1941, 48, 316-336.

Peak, H., Brooks, J., and Hobson, B. Positive and negative practice in the correction of spelling errors. *J. Psychol.*, 1941, 11, 103-114.

Peak, H., and Deese, L. Experimental extinction of verbal material. *J. exp. Psychol.*, 1937, 20, 244-261.

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the necessary relationships, and to solve the problem without trial and error, and presumably, without repetition.

[12] Prediction of the course of learning on the basis of the efficacy of a single trial cannot be made, however, since factors associated with the sequence of trials (work, transfer, and distribution of practice phenomena) play a role in this determination.

Peterson, J. Learning when frequency and recency factors are negative. *J. exp. Psychol.*, 1922, 5, 270-300.

Peterson, J., and Allison, L. W. Effects of visual exposure on the rate and reliability of stylus maze learning. *J. gen. Psychol.*, 1930, 4, 36-46.

Pratt, C. C. Repetition, motivation, and recall. *Brit. J. Psychol.*, 1936, 26, 425-429.

Rubin-Rabson, G. The influence of analytical pre-study in memorizing piano music. *Arch. Psychol., N. Y.*, 1937, 31, No. 220.

Sartain, A. Q. The conception of the true path and efficiency in maze learning. *J. exp. Psychol.*, 1940, 26, 74-93.

Seashore, H., and Bavelas, A. The functioning of knowledge of results in Thorndike's line-drawing experiment. *Psychol. Rev.*, 1941, 48, 155-164.

Thorndike, E. L. *Human learning*. New York: Century, 1931.

Thorndike, E. L., *et al. The fundamentals of learning*. New York: Teachers College, Columbia Univ., 1932.

Wang, T. L. The influence of tuition in the acquisition of skill. *Psychol. Monogr.*, 1925, 34, No. 154.

Waters, R. H. The influence of tuition upon ideational learning. *J. gen. Psychol.*, 1928, 1, 534-547.

Waters, R. H. The influence of large amounts of manual guidance upon human maze learning. *J. gen. Psychol.*, 1930, 4, 213-227.

Waters, R. H. The effect of incorrect guidance upon human maze learning. *J. comp. Psychol.*, 1931, 12, 293-301.

IX

TRANSFER OF TRAINING

INTRODUCTION

TRANSFER of training occurs whenever the existence of a previously established habit has an influence upon the acquisition, performance, or relearning of a second habit. It is one of the most general phenomena of learning and, by means of its influence, almost all learned behavior is interrelated in various complex ways. Transfer serves to determine, in part, the ease of learning of a particular habit, and, indeed, every new learning takes place in the context of all previously established habits. Moreover, as we have seen, in the learning of complex acts, improvement in one portion of the habit may transfer to facilitate or to inhibit improvement in other portions. Thus, the serial position curve in rote learning, the order of elimination of blinds in maze learning, and a number of similar phenomena are, in part, a function of transfer.

Transfer effects may be (a) *positive*, when training in one activity facilitates the performance or acquisition of a second activity, (b) *negative*, when the training in one inhibits or retards the learning or performance of another, and (c) *zero* or *indeterminate*, when training in one has no observed influence on the performance or acquisition of a second. The great majority of the experiments on this problem have obtained either positive or negative transfer with positive transfer predominating. Few have yielded zero transfer, partly because the search has been primarily for transfer of one sign or the other, with the result that the activities used have been chosen because they seemed likely to show transfer of one sign or the other. Strictly speaking, to demonstrate the occurrence of zero transfer would require proof of the null hypothesis, a feat which experiment cannot accomplish (Fisher, 1935; Melton, 1936). There are, however, some activities between which one would

expect, on theoretical grounds, to find little or no transfer, and practically one may infer that none exists when every reasonable effort has been made to demonstrate transfer without result.

The action of transfer in the laboratory and in more practical learning situations is inescapable. Few "new" materials or activities are wholly new to a person beyond the earliest period of life with the result that past experience may influence present learning in either a positive or a negative manner. New learning problems are usually solved in terms of previously acquired methods of attack or in terms of previously acquired specific acts. Furthermore, the changed associative organization which each new instance of learning brings about becomes, in turn, a determiner of future learning.

Examples of the transfer effect are not difficult to find. Practice on one maze or rational problem influences the rate of learning of another. Practice at learning one kind of verbal material influences the rate of learning another kind because the general method of learning is the same. Knowledge of one foreign language helps, or perhaps hinders, in detail, the acquisition of a second. Wherever knowledge is organized in a hierarchy (for example, arithmetic to algebra or mathematics to physics), the transfer of training from lower to higher levels is apparent. The personal and practical problems of daily life, whether evaded, half solved, or faced and mastered, are met in terms of transfer from the ways other problems have been treated in the past. Similarly, emotional habits and attitudes, established with respect to one person early in life, may be shown to transfer to later life situations in terms of the appearance of similar responses toward other individuals without opportunity for new learning to have taken place. Positive transfer, in fact, is a major facilitating device or "short-cut" in mental organization. It is probably the major condition of the empirical phenomena of understanding and insight. Negative transfer, on the other hand, is one of the major inhibitors of further learning and of retention.

While the importance of transfer of training should not be underestimated, it is certainly possible to place too great an emphasis upon the role it plays in the educative process. Thus, the doctrine of formal discipline as an educational theory assumed a type of trans-

fer and a generality of its operation which we now know does not exist. Only within the last few decades has sufficient experimental evidence been gathered to permit the transfer phenomenon to be analyzed and stated as a function of the conditions under which it occurs. Even today, we are unable to predict the transfer effect precisely except under certain limited laboratory conditions.

Methods of Measuring Transfer of Training

The measurement of transfer of training presents some of the most difficult methodological problems. Essentially, two problems are involved: the problem of experimental design and the problem of selecting an appropriate measure. Each of these problems, however, contains other complex difficulties which require solution if transfer of training is to be measured.

The simplest experimental design for the study of the transfer effect is indicated in the following diagram:

Design 1.

Control	. . . .	Learn habit 1
Experimental	Learn habit 2	Learn habit 1

Under these circumstances, amount of transfer (and its sign), is determined by comparing the performances of the two groups with respect to the learning of "habit 1." The method suffers from two deficiencies, however. In the first place, there exists no adequate basis for matching the control and the experimental groups. This is not a serious difficulty so long as we are merely interested in the occurrence of transfer rather than in a determination of its amount, since, if subjects are assigned to the groups at random, ordinary measures of significance may be employed to determine whether or not transfer has occurred. We would expect this design to lead to a less precise estimate of amount of transfer than could be obtained through the use of a matched group design. The degree to which precision is lacking can be precisely estimated, however, and this lack of precision can be decreased by the use of large numbers of subjects. The second objection to the use of this design concerns

the occurrence of the warming-up effect which has been noted by Thune (1950a) and Hamilton (1950). Thune's results indicate that, if a subject engages in a learning or warming-up activity before learning a second activity, the learning of the second activity is enhanced.[1] As we shall see, this effect is apparently independent of transfer of training in the ordinary meaning of that term since (a) little or no learning need occur on the first task and (b) the effect dissipates rapidly in time (Hamilton, 1950). Thus, in the design noted above, the learning of habit 1 by the experimental group would be influenced both by transfer of training from the learning of habit 2 and also by the beneficial effects of warming up derived from practice on habit 2. Although not enough is known about the warming-up effect to predict its influence in all situations, it is probable that this effect would cause an overestimate of positive transfer effects and an underestimate of negative transfer effects. This would be particularly true if the learning of habit 1 followed immediately upon the completion of practice on habit 2. Certainly, the results obtained by Thune and Hamilton emphasize the need for specifying the interval between the two practice sessions (longer intervals being more desirable than shorter ones) if this type of design is to be used.

A second type of design is illustrated below:

Design 2.

Group 1	Learns habit 1	Learns habit 2
Group 2	Learns habit 2	Learns habit 1

Under this plan, each group serves as a control for the other and the data are combined to produce the estimate of transfer. This design again presents the difficulty that no basis for matching the groups exists. It does, however, control more or less closely the warming-up factor (if it is assumed that activity 1 is as effective a warming-up activity for the learning of activity 2 as activity 2 is for the learning

[1] Additional evidence for the effectiveness of this factor can be found in the studies of proactive inhibition by Underwood (1945, 1949). In this case, increased amounts of prior learning cause a relative facilitation of subsequent learning, even though the conditions of similarity are such as to promote negative transfer.

of activity 1). The primary difficulty with this design is that it assumes that habit 1 will transfer to habit 2 in an amount equal to the transfer in the opposite direction, an assumption which has not received independent verification, and, in any case, would require verification over a wide range of learning tasks.

A third type of design, and one which is most widely used, is outlined below:

Design 3.

Control	Pre-test on habit 1	. . . .	Learn habit 1
Experimental	Pre-test on habit 1	Learn habit 2	Learn habit 1

In this case, again, transfer is estimated in terms of a comparison between the groups on the final learning of habit 1. Design 3 possesses the advantage that a basis for matching the groups exists. This matching may be accomplished either by manipulating the samples to equality on the basis of the pre-test scores so that precise determinations of amount of transfer can be made, or the pre-test scores may be used to increase the precision of the significance estimate by means of analysis of covariance. The warming-up effect remains uncontrolled, however, although we have seen that this effect may be minimized by increasing the time interval between the learning of habit 2 and the learning of habit 1 in the experimental group. It should be recognized that this design does not yield a pure measure of transfer since the final performance in both groups is, in part, determined by the learning which takes place during the pre-testing procedure.

The designs described above may be used with a variety of measures. At the simplest level, initial performance on the final test, number of trials to reach some criterion on the final test, or any other performance measure applied to the final test may be used. At this level, no difficulty exists in specifying the sign and amount of transfer of training, since this specification is accomplished in terms of a comparison of raw scores between the experimental and control groups. On the other hand, if it is desired to express transfer in terms of a meaningful percentage figure, and particularly if it is desired to compare amounts of transfer obtained

under a variety of experimental conditions, no entirely adequate measure of transfer exists. In their excellent article, Gagné, Foster, and Crowley (1948) have reviewed the traditionally employed measures of transfer. They conclude that the expression of transfer in terms of the percentage of total possible improvement which is provided by transfer furnishes a measure which is comparable from one transfer situation to another. While there are certain limitations to this procedure, there is no doubt that this method is the best one currently available for the measurement of transfer.

Characteristic Phenomena of Transfer

While the definition of transfer offered in the first sentence of this chapter will suffice as a general statement, its meaning should be amplified and made more specific. Practice results in effects other than learning, and the transfer of these effects from one learning situation to another should be disregarded in the study of transfer of training insofar as possible. At the very least, an attempt should be made to consider these other types of transfer effect separately. Thus, fatigue and work decrement effects often result from practice and will unquestionably transfer from one learning situation to another if the time interval between the practice periods is sufficiently short. These work decrement effects, however, accumulate, dissipate, and probably show transfer, under different conditions from habit effects. Their influence should be analyzed out of the results on transfer of training. Similarly, the recent work on the warming-up effect indicates that it would be unwise to ignore this factor in the study of transfer. Increase in the warming-up effect may be relatively independent of the conditions of the learning process and its decrease may not correspond to forgetting in the usual sense of that term. It is unquestionably prudent, at the present time, to consider warming up and transfer of training as separate phenomena, even though we may feel that future work will show them to be intimately related.

Concerning transfer of training itself, a few words should be said. Situations for the study of transfer have differed on the gen-

erality of the transfer effect being studied. Some of the experiments, those dealing with stimulus generalization or the transfer of specific responses in rote-learning, for example, have dealt with an extremely specific type of transfer. Other studies—those that relate to the concept of formal discipline are an example—have dealt with a far more general form of transfer. Between these extremes are found some of the studies on transfer resulting from practice upon successive samples of the same kind of material, some of the motor learning studies of transfer, and others. It is important to realize that different kinds of information are yielded by these studies. The more general transfer situations have typically been oriented toward a solution of some practical educational problem while the more specific situations have been concerned more with the isolation of the conditions under which transfer occurs or with a test of some theoretical deduction.

Stimulus Generalization as a Form of Transfer

The simplest form of transfer of training, and one which serves as a theoretical basis for the explanation of many of the more complex forms of transfer, is the phenomenon of stimulus generalization. This effect has been described in some detail in Chapter III. The empirical phenomenon of stimulus generalization is defined by the fact that a response which has been conditioned to a particular stimulus tends to occur when a second stimulus, similar to the first, is presented. The amplitude of such generalized conditioned responses tends to decrease as the degree of similarity between the original stimulus and the test stimulus decreases. As it is usually measured, stimulus generalization represents positive transfer in the sense that the generalized response occurs without or with less training than would be required to produce that response in the absence of prior conditioning. As we shall see, all transfer of training is positive in this sense. Negative transfer is a phenomenon which occurs as an artifact of the transfer situation when the to-be-learned response is changed between the time of original learning and the measurement of the transfer effect.

Learning To Learn as Transfer of Training

A number of studies have yielded information on the learning to learn phenomenon. This effect is noted when the learning of successive samples of material of the same kind is accompanied by an increase in the speed of learning. Thus, the results obtained by Ward (1937) show a decrease in the number of trials required to learn lists of nonsense syllables as a number of such lists are learned in succession (Fig. 32).

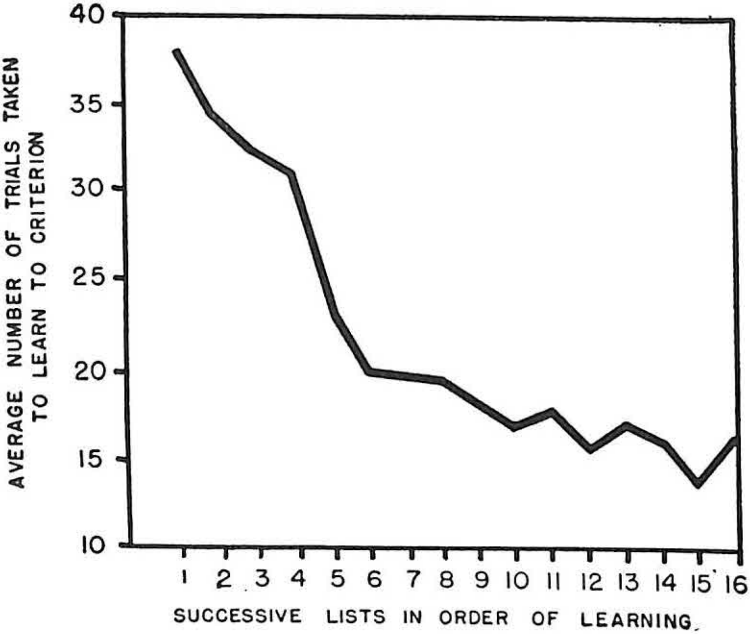

FIG. 32. A CURVE SHOWING THE CHANGES IN RATE OF LEARNING 16 SUCCESSIVE LISTS OF 12 NONSENSE SYLLABLES
(From Ward, *Psychol. Monogr.*, 1937, 49, p. 13)

That this effect may still be measured after considerably greater amounts of practice is implied by the findings of Melton and Von Lackum (1941). The subjects in this experiment had previously served for twenty-eight days in an experiment in which lists of syllables, words, and numbers had been learned. Nevertheless,

further practice effects appeared in the Melton and Von Lackum experiment.[2]

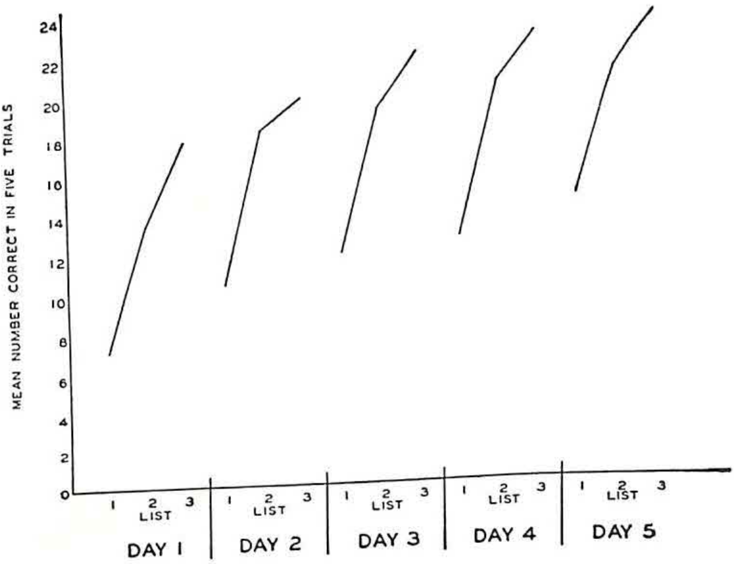

FIG. 33. LEARNING TO LEARN AND WARMING-UP EFFECTS IN ROTE LEARNING (From data furnished by Professor Thune. Cf. Thune, 1950b)

Subjects learned three different lists each day for five days. The gains between days are largely attributable to learning to learn while the gains within days are largely attributable to warming up.

In a recent study, Thune (1950b) has demonstrated the differential effects of warming up and learning to learn. In this experiment, Thune's subjects learned three ten-unit lists of paired associate adjectives on each of five successive days. The results are shown in Figure 33. If gains within days are considered as a function of

[2] The wide range of increments in performance that occur as a subject passes from sample to sample of a single class of material may be illustrated by a selection from the many examples in the literature. Increments appear in the forms of immediate memory tested by visual apprehension (Dallenbach, 1914) and memory span (Martin and Fernberger, 1929) experiments, in a wide variety of psychophysical experiments (cf. Fernberger, 1916), in situations requiring accurate visual perception and later report (cf. Whipple, 1921), in mental multiplication, even by already well-practiced subjects (Thorndike, 1908), in problem-solving and rational learning of many kinds (cf. Ruger, 1910; McGeoch and Oberschelp, 1930), and in performance on successive intelligence tests (Dunlap and Snyder, 1920; Thorndike, 1922).

both warming up and learning to learn, while gains between days are considered as measures of learning to learn, it is apparent that warming-up effects, while very influential, are quite transitory. Transfer gains, on the other hand, are smaller but more permanent.[3]

The very important work of Harlow (1949) also deserves mention in this connection. Working with monkeys, Harlow has clearly shown that the learning of a particular discrimination problem is a function of the number of previously learned problems of the same general sort. Monkeys solving the first few problems acquire skill slowly. After having solved several hundred problems of a similar nature, however, learning is extremely rapid. A clearer example of the cumulative effects of transfer cannot be found in the experimental literature. Not only are Harlow's results important on this account, but also they afford an explanation of the empirical phenomenon of insight. Harlow's monkeys showed insightful (i.e., sudden) learning of a particular problem only when they had solved many similar problems in the past.[4]

Learning to learn is a phenomenon which takes place concurrently with the learning of specific tasks. These increments are so commonly found that an experiment which did not reveal them would be noteworthy on that account. Most probably, learning to learn results from a transfer of general methods of attack and technique of acquisition from one situation to another, but it may also result, in part, from learning to reacquire the "set" appropriate to

[3] It may be argued, of course, that Thune's results show only learning to learn and that the loss between days represents a forgetting of this learning. This contention, however, is not supported by the results of Bunch (1936) indicating the relatively high resistance of transfer to forgetting, nor is it supported by the earlier results of Thune (1950a) on the conditions of the warming-up phenomenon or by Hamilton's (1950) demonstration of the rapid dissipation of warming-up effects in time. A crucial test of this matter could be made if it could be shown that negative transfer effects can exist after positive warming-up effects have dissipated.

[4] Harlow's results also suggest that the most efficient way to learn a particular problem may be (under certain circumstances) to learn a number of different, but similar, problems. This may be true in cases where the conditions of learning are inadequate to produce much improvement in the case of the final problem and are quite favorable in the preliminary problems. Indeed, this is one of the assumptions underlying the use of synthetic training devices in the armed forces and elsewhere.

that type of learning situation more rapidly (cf. Ammons, 1947a). The resulting increments enter as progressive errors in nearly every experiment which, although not designed to study learning to learn effects, must deal with the performance of the same subjects on successive tasks. Various counterbalancing procedures have been designed to compensate for these errors.[5]

Transfer from One Class of Material to Another

Early interest in transfer of training was focused upon this division of the problem. The proponents of formal discipline as an educational doctrine took the position that the results of training were general in the sense that the incremental changes resulting from practice in one activity carried over with but small loss to unpracticed activities of different kinds. It was to be expected, therefore, that the experimenters who first tested this educational theory should have worked with material from different classes. There is no reason to suppose, however, that the conditions underlying transfer from one class of material to another are fundamentally different from those which are basic to transfer among samples of the same class. Moreover, classes grade into each other, and there is often no sharp distinction between them.

The large number of studies which followed the pioneer investigation of Thorndike and Woodworth (1901) have corroborated and extended their conclusions that transfer may be either positive or negative and that there is usually less transfer from one activity to an activity of a different class than from one sample to another of the same class.

To call the roll of the extensive experiments would be fruitless, and only a few illustrations need to be mentioned. Transfer of rate of memorizing from one material to a different one, while often positive, is relatively small in amount (cf. Sleight, 1911; Reed, 1917).

[5] As Ward (1937) points out, ordinary counterbalancing procedures operate to compensate for the learning to learn phenomenon only if the curve of learning to learn is linear. Ward's own data do not support this assumption, however. For a general discussion of this and other deficiencies of the methods of counterbalancing, see Chapter I.

There are considerable amounts of positive transfer between writing English prose in German script and turning German script into English (Leuba and Hyde, 1905), from training in mental multiplication to performance in several other arithmetical operations, (Starch, 1911), from practice in canceling English words containing the letters *a-t* to performance in other forms of cancellation (Martin, 1915), from practice on a pictured representation to practice on a motor task (Gagné and Foster, 1949), and from one school subject to another (cf. Mudge, 1939; Stroud, 1940).[6]

Training on an activity of one class may also retard the rate at which an activity of another class is learned. Transfer has appeared from canceling the letters *e* and *t* to canceling meaningful words (Kline, 1914) and from canceling certain letters to canceling numbers (Martin, 1915), from learning to spell words to learning to spell their derivatives (as from "create" to "creating") (Archer, 1930), and in many other situations. Often this transfer appears overtly among the component parts of a complex habit as well as in terms of a net transfer effect.

In recent years, experimentation on this problem has virtually disappeared, having been supplanted by experiments having a more analytic or theoretically oriented approach to the study of transfer.[7]

CONDITIONS OF TRANSFER ANALYZED IN TERMS OF RELATIONS BETWEEN THE TRAINING AND THE TEST ACTIVITIES

The phenomena of transfer described thus far have been relatively molar and unanalyzed. It is important to know that training in one activity facilitates or inhibits the learning of another activity, but this alone does not specify the basic variables involved. The

[6] References to much of the earlier work and to some of the later will be found in Thorndike (1914), Coover (1916), Bills (1934), Woodworth (1938), and Gagné, Foster, and Crowley (1948).

[7] While this is true of those studies reported in psychological journals, it should be noted that many studies of this variety have been performed by psychologists in the military services and other applied areas. Thus, the evaluation of training programs, synthetic training devices, etc., is, in part, dependent upon studies of transfer of training. Reference to much of this work may be found in the series of Army Air Forces Aviation Psychology Program Research Reports (1947-1948).

most significant variables have turned out to be the relations between the training and the test activities. These have been analyzed in terms of the relations between the stimulating conditions in the two cases, relations between the responses, and general factors which are applicable to both activities. A survey of these more analytic studies will make clearer what the phenomena of transfer are and, by stating some of its basic conditions, will set the stage for interpretation.

One obvious mode of analysis is to break down the training and test activities, the activities *from* and *to* which the transfer takes place, into stimulating conditions and the responses thereto, remembering always that these two are a continuous series. The subject is trained on one set of stimulus conditions and tested, not upon some total activity as was done in the unanalytic studies, but with a derived or altered arrangement of stimuli. This is done in order to discover whether, and to what extent, the original response will be elicited by the derived stimulus.

Relations of the Receptors Stimulated

One of the earliest questions asked in the field of transfer was whether training to make perceptual discriminations among stimuli applied to receptors on one side of the body will transfer to discriminations of stimuli delivered to receptors on the other side. The older literature contains several reports of positive transfer of discrimination, particularly on the skin. More recently, Hulin and Katz (1934) trained five seeing subjects, hitherto unfamiliar with the Braille alphabet, until each could tactually discriminate and name every letter with each of the five fingers of one hand. The subjects were then asked to recognize the letters presented in a random order to the corresponding finger of the other hand. Two subjects showed 100 per cent positive transfer, and none of the remaining three showed less than 88 per cent. Franz (1933) has reported positive transfer of form discrimination from one side of the body to corresponding points on the other side, and Mukherjee (1933) finds transfer in two-point discrimination from left forearm

to right. There is no doubt that under a wide range of conditions the effects of practice are not confined to the tactual receptors stimulated during practice, but that they transfer positively to bilaterally symmetrical areas.

A similar generalization holds for the retina. The results of Franz and Layman (1933) show a large amount of positive transfer of training in visual discrimination from right eye to left eye, and J. J. Gibson (1933) finds a transfer of the negative aftereffects of the perception of curved lines from stimulated to unstimulated eye. High positive transfer from one retina to the other is so obvious a fact of daily life as to need no further demonstration. An object recognized, a discrimination made, or an act determined by stimulation of one retina can be responded to with little or no decrement when the stimuli are shifted exclusively to the other retina. The positive transfer from one eye to the other is sufficiently great to render the eyes interchangeable receptors. The same is true for the ears and, probably, for other receptors located in bilaterally symmetrical positions.[8]

Generalization of Stimulating Conditions

The phenomenon of stimulus generalization is, as we have already noted, the simplest form of transfer of training. As it has been studied in conditioning situations, this effect has already received our attention in Chapter III. In situations which more nearly resemble those of the classical learning experiments, analogous results have been obtained. E. J. Gibson (1939), for example, determined a negatively accelerated gradient of generalization in a simple verbal learning situation. Her subjects were instructed to respond quickly with a single nonsense syllable to a specific vibratory stim-

[8] There is also some transfer across sensory modalities. Some of this may be regarded as stimulus generalization, but most of it is probably mediated by symbolic processes. The role played by these mediating processes in transfer between receptors, whether or not of the same modality, is of undetermined amount. An example of transfer across modalities may be found in the fact that objects with which one's first acquaintance is visual may be recognized by touch, and vice versa.

ulus on the back and not to respond to other stimuli. After practice with the reference stimulus, other vibratory stimuli were applied at intervals of four inches in a straight line, with the result that the response transferred with a decelerated gradient.[9]

The experiments of Yum (1931) are still more clearly in the tradition of the classical experiments. His learning materials were lists of fourteen paired associates in which the stimulus terms were hyphenated nonsense syllables and the response terms were four-letter words. Twenty-four hours after a list had been learned to a criterion of one perfect recall, a test for transfer was made with the syllables changed in the ways illustrated in Table XVI. Transfer,

TABLE XVI

TRANSFER AS A FUNCTION OF CHANGED CONDITIONS AT RECALL

(From Yum, *J. exp. Psychol.*, 1931, 14, p. 73)

Conditions	Original Stimuli	Response Words	Recall Stimuli	Per Cents Recalled
Same syllables (Control)	Reb-qim	Wolf	Reb-qim	67.76
1st letter of 1st syllable changed	Hud-lep	Fist	Xud-lep	40.15
2nd letter of 1st syllable changed	Toq-bex	Jury	Tiq-bex	59.85
1st and 2nd letters of 1st syllable changed	Kaj-zoy	View	Nej-zoy	39.38
1st letter of 2nd syllable changed	Vah-miz	Nose	Vah-piz	40.93
2nd letter of 2nd syllable changed	Wul-gic	Vase	Wul-goc	53.86
1st and 2nd letters of 2nd syllable changed	Jec-por	Mask	Jec-nar	37.65

measured by the percentage of correct recalls on the test, was uniformly positive, but varied in amount with the changes in the stimuli. Every alteration of the stimulus member reduced the recall by a statistically significant amount. Changes in the second letter of either stimulus syllable decreased recall less than did changes in the first letter or in the first and second letters. These results demonstrate that a response which has been associated with a

[9] The analogous experiment in classical conditioning is Bass and Hull (1934).

given stimulus may be aroused by a stimulus partially identical with the first, but with which the response has not been previously associated. The results also indicate that the extent to which this is true varies with the locus of the identity.

Similarity more often means resemblance rather than some locus or amount of identity, however, and it is fortunate that Yum carried his problem further by using lists of twelve paired meaningful words in the training series and by substituting for these stimulus words, after twenty-four hours, other words rated as having first or second degree similarity of meaning to the original words. An analogous experiment was done with visual patterns as the stimuli and three-letter words as the response terms. At recall, drawings of four different rated degrees of similarity to the original stimuli were substituted. All of the variations in stimuli yield some recall, but the amount increases with increasing similarity (Table XVII). Yum's

TABLE XVII

TRANSFER AS A FUNCTION OF RATED SIMILARITY BETWEEN STIMULUS MEMBERS

(From Yum. *J. exp. Psychol.*, 1931, 14, pp. 75 and 78)

Condition at Test	Percentage Recalled	
	Word Stimulus Similar	Visual Pattern Similar
Word or pattern same	50.15	84.62
First-degree similarity	32.56	64.53
Second-degree similarity	11.27	49.15
Third-degree similarity		45.30
Fourth-degree similarity		36.32

findings have recently been corroborated by E. J. Gibson (1941). Comparable results have also been obtained by Gulliksen (1932) when degree of similarity was measured objectively in terms of the size or direction of angles while variations as a function of the subject's knowledge that he was being tested were controlled.[10]

The so-called *transposition* experiments are continuous with those

[10] Cf. also Razran's (1939) ingenious test of transfer in human conditioning, where he finds a greater positive transfer to synonyms than to homophones. Razran (1940, 1949a, b) should also be consulted.

described above in that, after practice with one set of stimuli, a definite change is made in the stimulating conditions and the subject is tested to discover whether he will respond in a similar way to the altered stimuli. In these experiments, however, the change is made in such a way as to leave similar relations between the stimuli. Since the early experiments were done on animals, the procedure may be illustrated by two experiments on animal subjects. After having trained chicks to choose a 6-centimeter circle and to reject a 4-centimeter circle, Bingham (1913) presented the chicks with circles of 4-centimeter and 3-centimeter diameters. They chose the 4-centimeter circle, the one which had previously been rejected. On the other hand, when the test stimuli consisted of 6-centimeter and 9-centimeter circles, the chicks chose the 9-centimeter one. In both cases, it was *as if* the chicks were responding to the relationship *larger than*. Two years later, Köhler (1915) reported similar experiments, which, by virtue of the interpretation given to them by gestalt theorists, have become the best-known instances of transposition. Hens were trained, for example, to eat from the darker of two gray surfaces and to react negatively to the lighter. The animals were then presented with this darker gray and with one still darker, whereupon they chose the new and still darker gray a majority of the time, while rejecting the stimulus which had previously been chosen.[11]

Concerning the existence of the transposition phenomenon there can be no doubt. The interpretation of this phenomenon, however, is a matter for some dispute. In cases where transposition occurs, it appears, superficially, that the subjects are responding to the relationship between the stimuli. The gestaltists view this as a response to the whole properties of the stimulating situation and a transposition of response to transposed whole properties. Spence

[11] Other representative studies of transposition in animal experimentation are Helson (1927), Gayton (1927), Warden and Rowley (1929), Perkins and Wheeler (1930), Lewis (1930), Warden and Winslow (1931), and Gundlach and Herington (1933). Klüver's (1933) monograph contains a large amount of evidence on transposition by monkeys in the visual, auditory, and somesthetic modalities, and demonstrates that transposition may take place in spite of relatively radical changes in other features of the situation.

(1937b), on the other hand, has proposed a theory which accounts for the appearance of transposition in terms of the algebraic summation of the excitatory and inhibitory tendencies aroused by the stimuli involved in the discrimination. According to this theory, both the positive (reinforced) stimulus and the negative (extinguished) stimulus generalize in such a way that transposition tests with similar pairs of stimuli will result in the occurrence of transposition. The important deduction is made, however, that transposition will not result if stimuli remote from those used in the original training are used in the test. Such a failure of transposition to occur when dissimilar pairs of stimuli are employed has been demonstrated by Spence (1937a, 1941) and others.[12]

Transposition experiments with human subjects reveal a corresponding transfer (cf. Jones and Dunn, 1932; Jackson, Stonex, Lane, and Dominguez, 1938; Wesman and Eisenberg, 1941; and Jackson and Jerome, 1943). There are conditions under which transposition does not occur, and even under favorable conditions there may be subjects who will not show it, but under many conditions and with many subjects, it does appear. One of the most significant studies of the transposition phenomenon using human subjects is the one by Kuenne (1946). She demonstrated a relationship between mental age and the occurrence of transposition behavior in children. Kuenne studied transposition behavior on near and remote pairs of stimuli with children who ranged in mental age between 36 and 83 months. At all mental age levels, transposition to a high degree was obtained when the stimuli used for testing were near to those used in original training. For the remote test stimuli, however, mental age was a significant variable. Those subjects with a mental age at the 42-month level responded in a chance fashion on the transposition tests, that is, transposition did not occur. As mental age increased, percentage of transposition responses increased until a level of 100 per cent transposition was obtained at a mental age level of 76 months. Kuenne interprets her results to

[12] Gulliksen (1932) and Klüver (1933) report such results. The student will also find it profitable to examine the theory of discrimination learning proposed by Gulliksen and Wolfle (1939).

mean that the theoretical formulations of Spence (1937b) apply to the transposition behavior of very young children, but that, with the development of language, transposition behavior becomes mediated by symbolic responses such as "bigger than" or "darker than." Kuenne's results offer an explanation of "relational" learning in terms of symbolic mediation and are opposed to the gestalt-type approach to this problem.

The facts cited in this section are simple or complex examples of stimulus generalization, the general principle being that *a response learned to a particular stimulus tends to be elicited by other, similar stimuli, the degree of this tendency being an increasing function of the similarity of the two stimuli.*[13]

As a first corollary to this principle, it is apparent that in many "new" learning situations, the stimulating conditions tend to elicit responses which have been connected with similar stimulating conditions in the past. Because of this, subjects usually enter a problem situation with a previously learned hierarchy of responses which, in part, determines the type and the variability of behavior in the new problem situation (cf. Hull, 1930, 1934). The principle of stimulus generalization provides a mechanism whereby training not only connects stimulus A with response B, but also connects B with other stimuli which are already related in some way to A. The new and altered stimulating conditions seldom elicit the response to the same degree as does the stimulus condition which prevailed during original training, but neither do they fail to elicit it in some degree. The transfer, that is to say, is positive but not equal to direct practice.

[13] This general principle has been noted under many different names such as associative spread, associative generalization, the law of similarity, the law of assimilation, and so on (cf. Carr, 1925). On the basis of evidence available as long ago as 1914, Thorndike (1914) could state such a principle and has published evidence in support of it (1937). A discussion will be found in Robinson (1932) under the title of the "Law of Assimilation." E. J. Gibson's (1940) systematization of many facts of verbal learning in terms of the concepts of differentiation and generalization involves the data surveyed here. More recently, the treatment by Hull (1943) is especially significant.

Relations between the Stimulating Conditions in the Two Activities: Positive Transfer

It is to be expected that a transfer effect which appears on a relatively brief test of performance will also influence continued learning, but this expectation requires experimental evidence to determine its validity. In an important attack on this problem, Bruce (1933) used as the training material lists of paired nonsense syllables and then tested the subjects on lists constructed of syllables having certain specified relations to those in the training lists (Table XVIII). Transfer was measured when the test list required learning

TABLE XVIII

A SUMMARY ILLUSTRATING THE EXPERIMENTAL CONDITIONS EMPLOYED BY BRUCE

(From Bruce, *J. exp. Psychol.*, 1933, 16, p, 347)

Number of Condition	Training Series Stim. (S_1)	Training Series Resp. (R_1)	Test Series Stim. (S_2)	Test Series Resp. (R_2)	Relations Between the Two Series
I	req	kiv	req	zam	S_1S_2 identical, R_1R_2 different
II	bij	bic	bij	tab	S_1S_2 identical, S_1R_1 similar
III	mir	ped	mir	miy	S_1S_2 identical, S_2R_2 similar
IV	tec	zox	tec	zop	S_1S_2 identical, R_1R_2 similar
V	lan	qip	fis	qip	R_1R_2 identical, S_1S_2 different
VI	soj	soy	nel	soy	R_1R_2 identical, S_1R_1 similar
VII	zaf	qer	qec	qer	R_1R_2 identical, S_2R_2 similar
VIII	bes	yor	bef	yor	R_1R_2 identical, S_1S_2 similar
IX	xal	pom	cam	lup	all terms different

to make a new response to an old stimulus, an old response to a new stimulus, and a new response to a new stimulus, as well as when the other relations schematized in Table XX obtained. The first list was given either 0, 2, 6, or 12 presentations, and the second, or test, list was learned to a criterion of 1 perfect trial. The results may be seen most readily when they are presented as the percentage that the mean trials to learn the second list is of the mean required to learn with no prior training. A percentage below 100, then, signifies positive transfer and one above 100 signifies negative transfer.

The results are so conditioned by the degree of learning on the original list that amount of transfer due to the relationships among the activities and amount of transfer attributable of prior learning must be considered together.

It will be seen in Table XIX that learning to make an old response to a new stimulus (Conditions V to VIII) uniformly yields positive transfer when the training list has been learned for 6 or 12 trials.

TABLE XIX

TRANSFER AS A FUNCTION OF STIMULUS AND RESPONSE RELATIONS AND A DEGREE OF LEARNING

(From Bruce, *J. exp. Psychol.*, 1933, 16, pp. 351-353)

	Condition	Mean Repetitions Required to Learn the Second Lists, When the First Lists Had Been Given 0, 2, 6, and 12 Repetitions, Expressed as Per Cents of the Mean After 0 Repetitions of the First List			
		0	2	6	12
I	S_1S_2 identical, R_1R_2 different	100	117	116	109
II	S_1S_2 identical, S_1R_1 similar	100	101	90	90
III	S_1S_2 identical, S_2R_2 similar	100	127	123	102
IV	S_1S_2 identical, R_1R_2 similar	100	102	101	80
V	R_1R_2 identical, S_1S_2 different	100	115	83	63
VI	R_1R_2 identical, S_1R_1 similar	100	103	81	77
VII	R_1R_2 identical, S_2R_2 similar	100	66	56	40
VIII	R_1R_2 identical, S_1S_2 similar	100	84	64	44
IX	all terms different	100	100	108	84

Two trials on the first list gives positive transfer only when the stimulus member of the test pair is similar to the common response member (Condition VII) and when the two stimulus members are similar (Condition VIII). Under these two conditions, the amount of the transfer is high, and under all four conditions the amount increases as the frequency of repetition on the training list increases. When all terms are different, the transfer becomes positive only with 12 trials on the training list. Discussion of Conditions I to IV will be delayed until the section on negative transfer.

Not only is learning to make an old response to a new stimulus

a condition which favors positive transfer, but the amount of that transfer is a function of similarities among the associated terms. The amount of transfer is also a function of the amount of training given on the first list, increasing as training increases from 2 to 12 trials. The basic conclusion that connection of a new stimulus with a response already associated with some other (but presumably similar) stimulus yields positive transfer has been verified by R. J. Hamilton (1943) and, in a trial-and-error learning situation, by Langer (1937).

Relations between the Stimulating Conditions in the Two Activities: Negative Transfer [14]

Analysis of the stimulus relationships involved in the occurrence of positive transfer has revealed that positive transfer results when the learner is required to learn to make an old response to a new stimulus. In the case of negative transfer, corresponding analyses have revealed that *learning to make a new response to an old stimulus yields negative transfer.* The experiments on associate inhibition are a case in point. In them the subject is required to associate a common term, or "old" stimulus, first with a response in the training series, and then with another response in the test series. (First A is associated with B and then A is associated with K.) Conditions I to IV of the experiment by Bruce (1933), which has already been described, are organized according to this associative inhibition paradigm (Table XVIII). All four conditions yield negative transfer after only 2 trials on the original list (Table XIX). Two of them, Condition I, where both responses are different, and Condition III, where the stimulus and response members of the test list are similar,

[14] In a sense, all negative transfer is an example of proactive inhibition. On the other hand, most studies of proactive inhibition have employed a type of experimental design different from those discussed in this chapter and more analogous to the design for studies of retroactive inhibition. Underwood (1945) discusses this matter and differentiates between *proactive inhibition in learning* (the type of negative transfer discussed here) and *proactive inhibition in recall* (studied with the other type of design). For this reason, the arbitrary decision was made to include the work on proactive inhibition in recall in Chapter X instead of in this chapter.

continue to show negative transfer after both 6 and 12 training trials on the first list, though in decreasing amounts. Two other conditions, II, where the stimulus and response members of the training list are similar, and IV, where the response terms of the two lists are similar, exhibit a shift toward positive transfer as amount of original training increases.

The similarities in Bruce's experiment are of one degree only. A later experiment by E. J. Gibson (1941), employing the associative inhibition paradigm (Training: A-B; Test: A-K), has used identical stimuli in the test list, and also three degrees of generalization of the stimulus items. Gibson's stimuli are visual forms, the similarities among which have been determined by the ratings of judges, as in the experiment by Yum (1931). Degree of generalization was then determined by testing the subjects, who had already practiced standard lists, with variants of the stimulus forms in order to discover how frequently the verbal response to the standard form was elicited by the variant form. Subjects, given 5 practice trials with the standard list, were then given 5 trials on the derived or test list, which contained a form having a known average rated similarity. As similarity and amount of generalization decrease, so does amount of negative transfer (Table XX). The greatest, and the only statistically significant, amount of decrease occurs from identity of stimulus terms to the varying degrees of similarity, but there is a marked tendency toward a further decrease with diminishing similarity, especially in terms of total number recalled in five trials. This positive relation between similarity and amount of negative transfer is still more strongly shown in a second experiment by Gibson, where the mean trials to learn the second list decrease, without inversion, from 7.43 when the stimulus terms are identical to 3.93 when the terms are dissimilar.[15]

It should be noted in Table XX that there is a tendency for the number of overt transfers or intrusions from the training to the test list to decrease as the similarity of stimulus terms decreases. The fact that the absolute number of overt intrusions is small probably

[15] These data are supported by the findings on retroactive inhibition as a function of similarity (Chapter X).

means that much of the interference between the training and test lists is implicit. It is important here that, in a code-learning problem involving simple manual responses, Siipola (1940) found that partial responses in the direction of the first learned response make up more than two-thirds of all errors. The subject did sometimes make a complete reversion to the originally trained response, but such overt intrusions were much less frequent than were the partial errors. Similar results have been reported by Lewis, Shephard, and Adams (1949). These partial errors are analogous to the implicit intrusions which may be inferred from the subject's responses in verbal learning. In the verbal learning situation, however, such implicit intrusions are much more difficult to measure.

TABLE XX

MEASURES OF LEARNING A SECOND LIST THAT HAS DIFFERENT DEGREES OF SIMILARITY FROM A TRAINING LIST

(From Gibson, *J. exp. Psychol.*, 1941, 28, p. 101)

	Relations Between Stimuli Members of the Two Lists			
Measure	Identical	1st Deg. Sim.	2nd Deg. Sim.	No Sim.
Mean number recalled at Trial 5	7.67	9.68	10.00	9.82
Mean total number recalled	22.75	30.91	31.82	33.95
Mean number intrusions from 1st list	.83	.45	.41	.41

Results supporting the conclusion that learning to make a new response to an old stimulus yields negative transfer have frequently been obtained with animals. Using a T-shaped discrimination box, Hunter (1922) trained rats to go to the right in response to light and to the left for darkness. When the animals had attained a criterion of 95 per cent errorless responses on two successive days, the directions were reversed, and the animals were trained to go to the left for light and to the right for darkness. The second habit, which required the learning of a new response to an old stimulus, was learned in an average of 603 trials. The first habit, on the other hand,

was learned in an average of 286 trials. Similar results have been obtained by Bunch (1939) in the maze learning situation.

Summary of the Findings on Transfer and the Stimulus Relationships

The data which have been discussed may be succinctly summarized. We have seen that learning to make a new response to an old stimulus yields negative transfer while learning to make an old response to a new stimulus yields positive transfer. We have also seen that these effects increase with increasing similarity of the stimulus components. Strictly speaking, then, we should expect maximum transfer when learning to make an old response to an old stimulus, that is, under conditions where we continue practice on the original task. It is apparent that this is true, but such a state of affairs is not usually regarded as a situation for the study of transfer. It is, of course, possible to regard the learning curve as a cumulative transfer curve but, if we exclude this situation, we find positive transfer when we learn to make an old response to a new stimulus. This facilitation effect will depend, however, on the degree of similarity between the old and new stimuli. As this similarity increases, we should expect to obtain increasing amounts of positive transfer. In general, transfer of training without regard to sign (whether positive or negative) is an increasing function of the stimulus similarity between the training and test activities. The sign of the transfer, however, depends upon the response relationships. If the response learned in the original training is one which will be adequate in the test situation (achieve the reinforcement, be scored "right," etc.), transfer will be positive. On the other hand, if the originally learned response will not prove to be adequate in the test situation, transfer will be negative.[16] The quantitative relation-

[16] Because of the nature of scoring in verbal learning situations, amount of positive transfer is generally somewhat restricted. Verbal responses are typically scored "right" or "wrong," hence the only adequate responses which can be learned in the original training is the *same* response as the one used in the test. This restriction is artificial and does not apply to learning situations where scoring is not accomplished in an all-or-none manner.

ships involved have been summarized by Osgood (1949) in the surface shown in Figure 34. It will be noted that, for any given degree of response similarity, amount of transfer (either positive or negative) increases as a function of stimulus similarity. In the same way, for any given degree of stimulus similarity, whether the transfer will be positive or negative (as well as amount of transfer) depends upon the degree of response similarity. Osgood's paper, giving the empirical derivation of this surface and an extended description of it, will repay careful study.

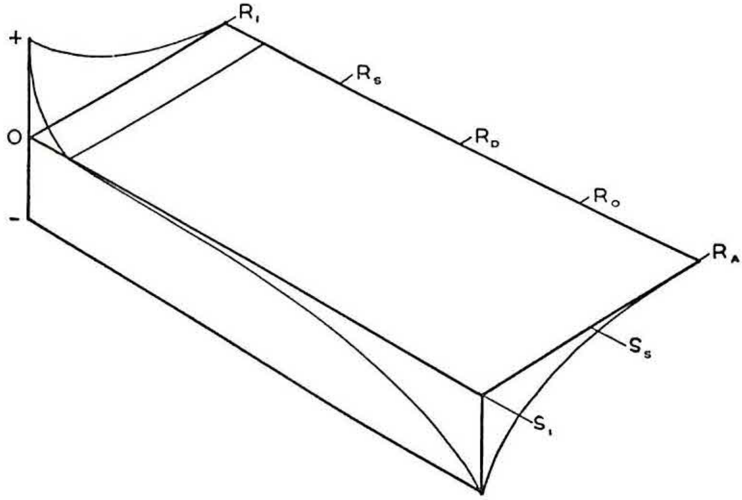

FIG. 34. THE TRANSFER AND RETROACTION SURFACE
(From Osgood, *Psychol. Rev.*, 1949, 56, p. 140)

The medial plane represents effects of zero magnitude. Response relations (ranging from response identity—upper left—through response similarity, dissimilarity, opposition to antagonism—lower right) are distributed along the length of the solid. Stimulus relations are distributed along its width.

Relations between the Responses in the Two Activities

As we have just noted, the demonstration that learning to make an "old" response to the same or a similar stimulus yields positive transfer, while learning to make a "new" response to the same or a similar stimulus yields negative transfer, tells us something about the relations of the responses in the training and the test activities

as well as something about the stimulus relations. When the response is "old," it is at or near the identity end of the continuum of similarity; when it is "new," it lies somewhere toward the zero end of this dimension. The problem of the influence of relations between responses is carried farther, however, in the work on bilateral transfer, sometimes called cross-education, and on transfer from one responding organ to another. Work on this problem began with the question, whether, and to what degree, training of a member on one side of the body, such as the right arm, will transfer to the bilaterally symmetrical member. The question has now been broadened to include transfer from the training of one responding member to the performance of another—that is, to the general problem of transfer of responses.

In these experiments, the stimulating conditions are the same in the test and in the training situations, and the results to be produced, or the acts to be performed by the subject are the same, but the responding organ and/or the specific responses differ. The situation resembles that of the Müller-Schumann paradigm of associative with one important difference. In associative inhibition, the old stimulus is to be connected with a new response in the sense of a new act or a new performance. In this case, however, the old stimulus is to be connected with the same act or achievement performed by a different part of the body and, to that degree, by means of new specific responses.[17]

Positive bilateral transfer from one hand to the other has been measured in such activities as hitting a target seen in a mirror, mirror-drawing, maze-running, tapping, pursuitmeter learning, and in many other perceptual-motor acts. It occurs in amounts varying from a small percentage to a very large one and over so wide a range of activities as to be a very general phenomenon (cf. Bray, 1928; Woodworth, 1938). It is not limited to bilaterally symmetrical members, but has been found from hand to foot on the same side (unilateral transfer), from left foot to right hand, and between

[17] The Müller-Schumann paradigm of associative inhibition may be stated as follows: When any two items, as A and B, have been associated, it is more difficult to form an association between either and a third item, K.

other combinations of hands and feet (cf. Bray, 1928; Cook, 1933). This is akin to the generalization of responses found in conditioning where a subject, conditioned to respond to a stimulus with one organ of the body, will also respond with another organ.[18]

Experimental measurements are scarcely needed to give evidence of widespread transfer of response. Given training in an activity involving primarily one arm or one leg, an individual can perform that act, though less well, with another response member. Typically, less practice is necessary to bring such a transferred response to the criterion reached by the first member. Not only may transfer be obtained between different response organs, but the same act may be performed in alternative ways by the same response organs.

Such transfer phenomena are examples of *response generalization.* As an empirical phenomenon, response generalization may be defined as follows: *A given stimulating condition, once connected with a particular response, will also elicit other responses which are related in some way to the first, or trained, response* (cf. Robinson, 1932).

Concerning the existence of response generalization as an empirical phenomenon, there can be no doubt. It is in the interpretation of this fact that certain difficulties are revealed. The question is, does this response generalization occur as a function of some innate property of the organism to show this generalization, or does its occurrence depend upon the previous learning of some mediating process? Such a learned mediating process is clearly evident when we talk about response generalization from a particular word to its synonym (Osgood, 1946; Morgan and Underwood, 1950). The extensive experiments by Wickens (1943a,b, 1948) on avoidance finger withdrawal can be interpreted in this way. Wickens' learning situation involves training to make an extension response of the hand to avoid shock. The hand is then inverted so that a flexion response becomes adaptive, and a high degree of transfer is noted.

[18] Illustrative references will be found in Gibson, Jack, and Raffel (1932), Shipley (1933), Gibson and Hudson (1935), Kellogg and Walker (1938), and Wickens (1938, 1939). Negative evidence may be found in the book, *Cats in a puzzle box,* by Guthrie and Horton (1946). A discussion of transfer of response in conditioning will be found in Hilgard and Marquis (1940).

This response generalization, however, may be symbolically mediated, or it may depend upon the previous learning of equivalent behavior routes to a goal (Hull, 1935). At the present time, no good evidence exists to support the notion that response generalization occurs in the absence of relevant prior learning. On the other hand, the results of Guthrie and Horton (1946) may be interpreted as negative evidence on this point. In view of the potential usefulness of this concept, however, it would probably be unwise to discard it on the basis of the scanty evidence which now exists. A clear differentiation between response generalization based upon some generalizing property of the organism and response generalization based upon a mediating process involving prior learning should be made. The former we shall refer to as *primary response generalization* while the latter shall be termed *secondary response generalization.*[19]

Transfer of General Factors Common to the Two Activities

In this group of relations between the training and test activities fall those which are not specific to particular stimuli and particular responses. Here we are concerned with the transfer of general principles, modes of attack, and sets to perform, which the subject learns or assumes during the training series and applies during the test series. There is, perhaps, no clear division between these three. Recent evidence, however, suggests that the first two factors, the transfer of general principles and modes of attack, should be considered separately from the transfer of set to perform. Each of these, however, is a general factor, acquired in one situation and applied in another.

[19] This differentiation is analogous to the breakdown of the empirical phenomenon of stimulus generalization into *primary* stimulus generalization and *secondary* stimulus generalization. (See Chapter III.)

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The Transfer of General Principles

One of the best-known illustrations of the transfer of a general principle is Judd's (1908) brief report of the transfer of a knowledge of the principle of refraction. Two groups of boys in Grades V and VI practiced throwing darts at a target under twelve inches of water. One group was taught the principle of refraction, the other was not. During the practice under twelve inches of water the two groups were equal, but when the depth was reduced to four inches, the group which had been taught the principle adjusted rapidly to the new conditions, while the control group showed scant transfer. An analogue of this experiment has been published by Hendrickson and Schroeder (1941), who substituted shooting an air rifle at a submerged target for the dart-throwing employed by Judd. The basic results are the same. The subjects who had been instructed in the principle of refraction showed greater transfer than the uninstructed control subjects by an amount which increased with the completeness of the explanation. The controls yielded a considerable amount of transfer, however. The results differ from Judd's only in showing some positive influence of knowledge of the principle on the initial training.

The generalizations which may carry over from one activity to another are very numerous, the important fact here being that they do determine transfer. Principles, whether overtly formulated or not, influence the solution of successive rational problems (J. C. Peterson, 1920); they are an important vehicle of transfer in puzzle solution, especially when the subject has analyzed the similarities between the first problem and a second (Ruger, 1910); and learning a problem with understanding of the relations involved in it has higher transfer value than learning without understanding of these relations (Katona, 1940; cf. Melton, 1941).[20]

[20] The value of the concept of *understanding* in this context is doubtful. Operationally, understanding merely means that the individual is able to verbalize, more or less clearly, the principle involved. That transfer can occur from verbal knowledge or training to performance of a non-verbal nature is a common assumption, which has been demonstrated to be valid (in varying degrees) by Sackett (1934, 1935), Perry (1939), Rubin-Rabson (1941), Vandell, Davis, and Clugston (1943), and Baker and Wylie (1950).

The transfer of general principles itself doubtless depends upon the complex transfer of previous learning. Thus, while some of Judd's subjects were instructed in the principle of refraction and others were not, much of the prior learning which made instruction in the *principle* effective was not measured. Thus, training to understand words, training to accept instruction, etc., is a prerequisite to Judd's experiment. If we are merely interested in the matter of learning to throw darts at submerged targets, we would undoubtedly find it more economical in time and effort to teach dart-throwing at every conceivable depth than to teach the habits which are prerequisite to the learning of the principle. Fortunately, the habits which form a necessary background to the learning of this general principle also have an enormously wide applicability for the learning of other principles. Herein lies one of the most powerful adaptive advantages given to man through the development of language. Nevertheless, general principles are one of the most important vehicles of transfer. It is possible, however, that principles will transfer to situations where they are inapplicable and thus result in negative transfer (cf. Luchins, 1942). Furthermore, the acquisition of a principle does not guarantee that it will be used in other situations where it may or may not be applicable. Much quoted in support of this are results which show that neatness acquired in one kind of work will transfer to other kinds if it is taught as a concept or general principle, but not if it is taught as specific to one kind of work (cf. Bagley, 1905). The conclusion has frequently been reached that the results of school learning *may* transfer widely if taught as general knowledge applicable to other situations than the one in which they were learned, but that they *need not* transfer. They probably will not if they are taught as specific to the particular course material (cf. Coxe, 1924; Stroud, 1940). It is probable that the transfer of general principles follows the same laws as other forms of transfer, and that a general principle will transfer (either positively or negatively) if the situation in which it is applied is highly similar to the one in which it has been learned. It may be possible, however, to train individuals to apply general principles more widely if specific training is given in that form of behavior

and if the learner is taught to analyze similarities between old and new situations.

The Transfer of Modes of Attack

The carrying over of modes of attack, of general methods, from one activity to another constitutes one of the most subtle and important vehicles of transfer. It overlaps with the transfer of specific responses, the transfer of general principles, and the transfer of various "learning sets." Some of the evidence concerning this type of transfer has already been discussed under the heading of "Learning to Learn as Transfer of Training." In practice on successive lists of verbal materials by the anticipation method (take Ward's (1937) paper, already mentioned, as an example) the subject learns not only the specific words in the different series, but also how to follow the instructions, how to look for mnemonic cues, how not to be disturbed by failure on any given item, and, in short, how to learn in this situation. These modes of response apply nearly equally well to any verbal materials learned in this way. In learning successive problems of any class, analogous general methods are learned and carried over from sample to sample.

From his study of bilateral transfer, Bray (1928) concludes that methods, "tricks," and modes of adjustment are the primary media of transfer from one member to another. Of the same order is Munn's (1932) observation that practice with one hand taught the subject how to formulate the problem of what was to be done and that certain modes of attack would not work while others might. Clearly, these factors will transfer to performance with the other hand. Coordinate observations have been made on transfer from training in one activity to testing on another one. Coover's (1916) subjects were seldom able to report the exact determinants of transfer, but he concludes that one factor common to card-sorting and typing is the "habit of stripping the essential process of its adventitious accessories," together with a more adequate interpretation of the instructions and a better control of "attention."

In an experiment of the first importance, Woodrow (1927) has compared the transfer effects of two different methods of training.

Three groups were given six tests of memorization and, after an interval of four weeks and five days, six other tests similar in form but different in content. The tests were not highly correlated with each other. During the interval, a *control group* received no training, a *practice group* had routine practice in learning poetry and nonsense syllables with no instruction about methods of learning, and a *training group* had practice with these materials, plus extensive instruction in the techniques of memorizing. The conclusion is very clear that the instructions in memorizing gave the training group a pronounced advantage over the practice group. During the equal amounts of time spent in training by these two groups, the materials were the same, but, in the training group, some time was devoted to the exposition of methods. Similarly, Cox (1933) has shown that systematic instruction in methods of assembling and taking to pieces an electric lamp holder has much higher transfer value than does the same amount of time devoted to uninstructed practice alone. The instructions had to do with how to arrange the parts on the bench, the optimal modes of observation of the parts, and how to organize the movements involved in assembly. Analogous results have been achieved when specialized instruction has been given in methods of studying college course materials (cf. Pressey, 1928; Behrens, 1935), and in a host of semi-clinical and clinical training situations.

The variable of instruction in method is continuous with that of guidance in learning. Ludgate's (1923) subjects, who had been given various amounts of guidance in one maze, consistently learned a second maze in fewer trials than those who had received no guidance in the first. The guidance had a selective effect on the errors made in the second maze. In producing transfer from one ideational problem to another, Waters (1928) finds that some forms of guidance in the first problem are superior to unguided practice. The method of calling attention to the significant aspects of the problem yields clear positive transfer, giving a short statement of the principles yields a small amount, while information as to error evidently has no differential effect.

Instruction in analysis, abstraction, and generalization produces

a distinct positive transfer to problems requiring reasoning and understanding (Barlow, 1937). Likewise, training in outlining transfers positively to school subjects, influencing both mastery of specific content and performance in situations requiring reasoning (Salisbury, 1934). Training in the analysis and statement of definitions transfers positively to other situations (Meredith, 1927), while the transfer from training in reasoning provided by problems in arithmetic with the emphasis on the principles of solution is considerable (cf. Stroud, 1940).

The instances cited are but a few of the observed instances of transfer of methods of attack from one activity to another. Sometimes the transferred methods are explicitly formulated and applied by the learners, but more often they are not. It is a common thing for a subject to show high positive transfer in terms of method without being able to state explicitly the methods he has learned and used. Whatever the vehicle of transfer, explicit formulation by the learner of what is transferred is not necessary, and, indeed, is probably the exception rather than the rule. If any further evidence for this statement is necessary, it is provided by the fact that transfer occurs in animals, where adaptation to a maze situation, the formation of "learning sets" and other general factors are important vehicles of transfer (Dashiell, 1920; Jackson, 1932; Coppock and Mowrer, 1947; Harlow, 1949).

The Principle of Non-Specific Transfer

The data which have been presented above may be summarized in the statement of a general principle of non-specific transfer. *A factor, such as a principle or a method, which is non-specific to the training situation, tends to be elicited by similar situations.* The factors implied are general ones, and the statement could as well be called a law of general transfer or the principle of the transfer of general factors. The word, "non-specific," has been used to emphasize that the transferring factors, although learned in the training situation, are not specific to that particular situation or to the stimulus-response connections acquired therein.

The Transfer of Set

Transfer of set is a phenomenon related to the transfer of general principles and the transfer of modes of attack in that it, also, is a general factor. It probably follows different laws, however, and hence, should receive separate treatment. In the first place, several types of set may be distinguished. Conventionally, set has been used to designate the calling into play of a particular, and limited, set of previously learned responses. This transfer is inevitably selective. Instead of bringing to a new situation all of one's repertoire of responses, only a few tend to be elicited. Thus, having learned a list of words, the subject may now be required to learn a list of their opposites. McClelland's (1943) study of verbal discrimination learning is illustrative. Here, subjects were required to reverse a series of verbal discriminations either early or late in training. While the reversed groups were inferior to the non-reversed groups, the reversed group which had received more training before reversal showed less decrement than the group which was reversed earlier in training. Presumably, during original training the subjects learned both the correct and the incorrect members of the stimulus pairs to be discriminated. When required to reverse these discriminations (i.e., give the previously incorrect members of the pairs) the subjects were able to call these habits into play.[21] A second type of transfer of set is illustrated in the work of Thune (1950a) and Hamilton (1950). This work stems from a study of the warming-up phenomenon, particularly as this has been observed in motor learning situations (cf. Bell, 1942; Ammons, 1947a,b; Irion, 1949). "Set" in this context refers to the establishment of postural and attentive adjustments which facilitate the learning (or retention) of a particular activity (cf. Irion, 1948). This type of transfer may be illustrated by the results of Thune's (1950a) experiment. These data show that, following the learning of an unrelated list or following the performance of a color-guessing activity that was similar

[21] Other work in this general tradition has been reported by Lewin (1921, 1922), Norcross (1921), Crafts (1927), Schwarz (1927), Sanderson (1929), and Siipola (1935). The reader is also referred to the discussion of *set* contained in Chapter VI.

to the learning activity, subsequent learning was enhanced. Amount of improvement in the subsequent learning, furthermore, depended upon the amount of time devoted to the previous (warming-up) activity. Hamilton's (1950) results indicate that this effect is quite transitory, the great majority of the transfer effect being lost well within one hour.

On the basis of these data, a general principle concerning the transfer of set may be stated, as follows: *If, prior to learning, set to perform is induced, rate of learning should be greater than if this set had not been induced. Amount of this increase in the rate of acquisition should be an increasing function of the similarity between the set-inducing task and the learning task, of the amount of time devoted to the induction of appropriate set, and an inverse function of the time between the two activities.*

The possibility that set factors may operate to produce negative as well as positive transfer should not be overlooked. It is possible that, by warming up the subject on some incompatible activity, subsequent learning could be retarded just as, under the experimental conditions described by Thune and Hamilton, subsequent learning may be accelerated. With reference to this possibility, however, no experimental data exist.

Summary of the Finding on Non-Specific Transfer

General principles, modes of attack, and various set factors may transfer from one learning situation to another. The influence this transfer will have depends, of course, upon the appropriateness of the principles, methods, or sets to the second learning situation. In all cases, however, the transfer is non-specific in the sense that the particular stimulus-response connections in the two learning situations are relatively independent of this type of transfer. One difference between specific and non-specific transfer lies in the fact that specific transfer may be expected to manifest itself upon the first presentation of the second learning task (at least in some cases), whereas non-specific transfer can never do so. A difference may also be noted between the transfer of general principles and modes of

attack on the one hand, and the transfer of set on the other. Whereas both of the former types of transfer require that some formal learning occur during the training on the first activity, the transfer of set makes no such requirement.[22] A further differentiation between these types of transfer may be made on the basis of the temporary nature of the transfer of set. Because of this temporary nature of set transfer, it is probably of less practical importance than other types of transfer. Certainly, its importance is limited to transfer situations where one activity follows another in fairly rapid succession.

OTHER CONDITIONS OF TRANSFER

Amount of Training

Amount of transfer from previous training depends upon the amount of this training. This general fact is, of logical necessity, true. The nature of this relationship, however, has been indicated by a number of experiments. In the conditioning of the galvanic skin response, for example, as the number of reinforcements increases from eight to forty-eight, both the reinforced and generalized responses increase in amplitude. There is also some indication that the ratio of the amplitude of the generalized response to that of the conditioned response increases at first and then falls off slightly (Hovland, 1937c). In the salivary conditioning of human subjects an analogous result appears when a single conditioned stimulus is used, but when patterns of stimuli are used, generalization increases with continued training (Razran, 1940). With increasing amounts of training in a transposition experiment, transfer increases from an initially small amount to a high point, after which, with further training, it begins to decrease with some subjects at least (Jackson, Stonex, Lane, and Dominguez, 1938).

[22] In rote learning, Thune (1950a) obtained as much transfer of set by the use of a color-guessing activity as he obtained by means of the learning of an unrelated list. Irion and Wham (1951) report that the rhythmical naming of the first nine digits, in order, serves as a satisfactory warming-up activity for the rote serial anticipation learning of nonsense syllables.

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As the number of repetitions given the training list in Bruce's (1933) experiment increased, amount of positive transfer increased under the conditions which favored positive transfer. Under conditions which initially favored negative transfer, the sign of the transfer shifted from negative toward positive. Siipola and Israel

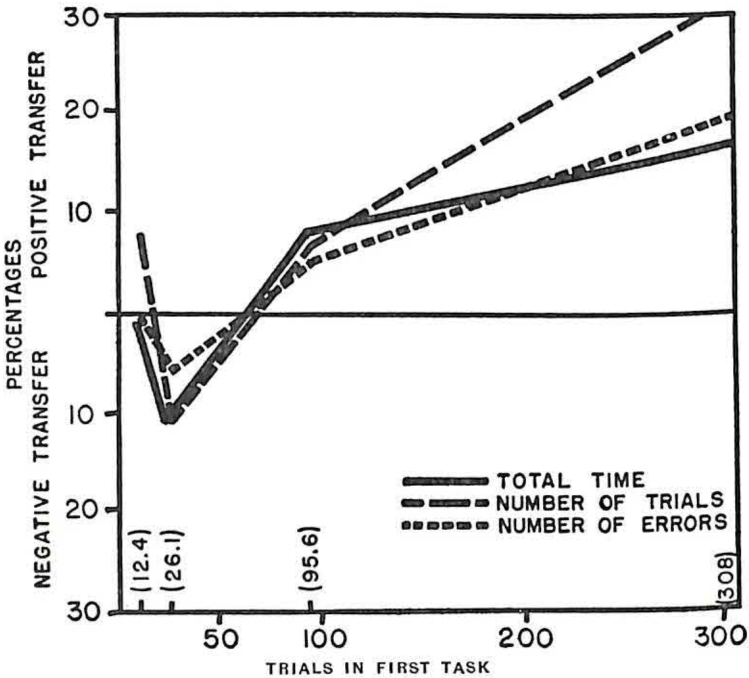

FIG. 35. PERCENTAGES OF POSITIVE AND OF NEGATIVE TRANSFER PLOTTED AGAINST NUMBER OF PRACTICE TRIALS IN THE FIRST TASK

(From Siipola and Israel, *Amer. J. Psychol.*, 1933, 45, p. 217)

Figures just above the base line indicate average trials for each of the four stages of training provided. The second or test task was regarded by the authors as having been "barely mastered."

(1933) have found, likewise, that in code-learning under conditions designed to yield interference, negative transfer appears with low degrees of training on the first task, but that this gives way to positive transfer when the amounts of training become greater (Fig. 35). Earlier, Bair (1902) had reported that, in simple activities, interference decreases as the learning of the initial habit increases, until

finally there may be a shift to positive transfer. By way of a different procedure, Kline (1921) arrived at a similar conclusion. This conclusion is also supported by the findings of Underwood (1945, 1949) regarding the learning of the second list in a proactive inhibition design following varying amounts of prior learning.[23] Over a considerable range of activities and conditions, then, conditions yielding negative transfer do so when the training activity is learned to a relatively low degree, but as training increases the amount of negative transfer diminishes and may be superseded by positive transfer. It is evident that this effect may be due, at least in part, to a transfer of warming-up effects from the prior training (cf. Thune, 1950a).

The data of Siipola and Israel suggest the further conclusion that the result is a function not only of degree of training on the original activity but also of the stage of practice on the material of the test. There is a strong suggestion that negative transfer is greatest when the degrees of learning of the training and test activities are similar (Siipola and Israel, 1933; Melton and Irwin, 1940; Sears and Hovland, 1941). The small amount of evidence available on positive transfer as a function of degree of original training shows it to increase as amount of training is increased (Ho, 1928; Cook, 1936).

In the case of non-specific transfer of set to perform, the same general relationship appears to obtain. Thune's (1950a) data clearly indicate that the amount of obtained transfer (of this kind) is a function of the amount of time devoted to the performance of the preliminary warming-up activity.

The Locus and Duration of Transfer Effects

When transfer is measured by the speed by which the test activity is acquired, for how long during practice on the test does this transfer effect continue to appear? In many cases of positive transfer, the trained subjects begin the test activity at a higher level of

[23] Relevant references to corresponding work with animals are the papers by Ho (1928), Wiltbank (1919), and Jackson (1932). Some of this effect may be due to a transfer of warming-up effects from the prior training (Thune, 1950a).

performance as a result of the preliminary training. This advantage over the controls continues for some time and may continue until the criterion is reached. Even if it disappears after a number of trials, the diminished time and errors during the early trials represent significant positive transfer (cf. Webb, 1917; Bray, 1928; T. W. Cook, 1936).[24]

Müller and Schumann (1894) have suggested that negative transfer (associative inhibition) might be a transient phenomenon which vanishes after a few trials. Several later investigators have found this to be the case. For example, Underwood's (1945, 1949) data on proactive inhibition are interpretable in this way. The extent to which this occurs may be a function of the relations between the training and the test activities, for Melton and Von Lackum (1941) have found that the percentage of proactive inhibition (negative transfer) decreases steadily from Trial 2 to Trial 5 in the learning of a second list of similar consonant syllables, while a dissimilar list shows no measured interference.

Practice in Successive Transfer Situations

The results of practice on more than two successive samples of the same class of material demonstrate that cumulative transfer effects appear at a decelerated rate. Since the days of Münsterberg (1889) and Bair (1902) it has been known that frequent shifts from one habit to another may cause the interference effects to decrease

[24] Webb's curves for positive transfer from one stylus maze to another show that the learning of the second maze begins at about the level attained on the fifth trial of practice without the presence of transfer, and that the locus of the transfer is largely confined to the first five trials. Hunter (1922) has objected to this conclusion on the ground that it does not follow when trials represent different fractions of the total learning effort required to reach a criterion. Hunter has applied a Vincent curve method to data of his own on negative transfer and has found that the experimental subjects begin at a lower level of performance, but that by the first half of the practice on the second activity the curves for the experimental and control groups are coincident. The use of Vincent curves shows the fraction of the total learning time during which transfer effects appear, but it may be questioned whether failure to use them invalidates the conclusions from data which have not been treated in this fashion. Rather, these techniques represent two different methods of representing the duration of transfer effects.

and sometimes to vanish altogether. This conclusion is illustrated also in the experiments on alternating activities (cf. Culler, 1912; Stroop, 1935), and in the learning of successive reversal problems (Harlow, 1949).

The fact that practice on successive interfering associations may decrease the interference suggests the conditions under which abstract concepts are formed. In concept formation a single feature, such as squareness, originally associated with many square objects, each in turn possessing different properties, becomes detached from the specific settings and is treated as an independent characteristic. The occurrence of abstraction in this manner, which James (1890, p. 506) has called the *law of dissociation by varying concomitants*, is, in part, an illustration of freedom from interference as a result of practice on varied transfer situations.

The Time Interval between the Training and the Test of Transfer

Any measure of retention, including the test of transfer of training, may be made at any time after the completion of original training. The curve of transfer as a function of time since training is especially interesting because the retention curves for the training material, as measured by the other measures of retention, are normally falling, and therefore the associative strengths of the originally learned material are less by these measures. Will amount of transfer also decline? This is the same as asking whether the subject's ability to use on one material what he has learned on another will decline with decreases in the otherwise measured retention of the first material.

Using as his training material one ten-letter Peterson Rational Learning Problem learned to a criterion of two perfect trials, and as his test material another ten-letter problem learned to the same criterion, Bunch (1936) has found that amount of transfer from the first problem to the second is independent of the lapse of time up to an interval of 90 days. One group of college students learned the second problem immediately after the first, while other groups

learned the second at intervals of 2, 14, 30, and 90 days after the first. By using still other groups, the retention curves for the first problem were determined by the saving method. The curves for saving in the first (training) problem fall, while those for transfer from this problem to the second one show no regular or significant change with time (Fig. 36). The two methods of measuring retention thus show different variations with elapsed time.

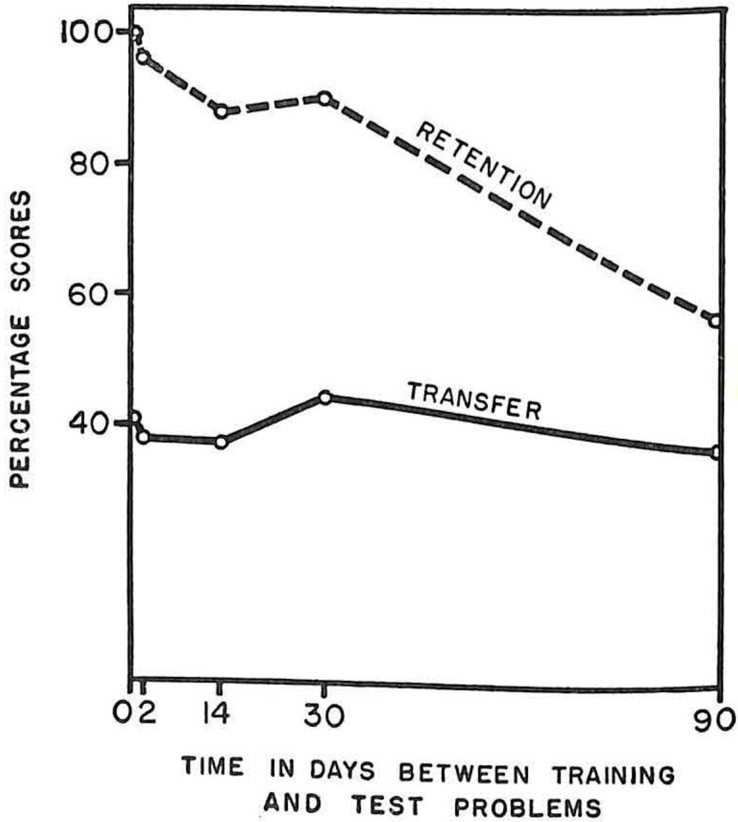

FIG. 36. SAVING IN FIRST RATIONAL LEARNING PROBLEM AND TRANSFER TO SECOND RATIONAL LEARNING PROBLEM (ERRORS)
(From Bunch, *J. comp. Psychol.*, 1936, 22, p. 332)

Bunch and McCraven (1938) have performed two similar experiments with paired nonsense syllables, allowing intervals of 0, 2, 14, and 28 days to pass between the first learned (training) list and the

second (test) list. Again, amount of transfer does not vary with time, and the transfer curves are, in effect, straight lines paralleling the time axis. The curves for percentage of transfer in terms of both time and errors rise slightly from an immediate test to a test after two days, thus suggesting a reminiscence phenomenon, but this increase is not significant.

In spite of the fact that the retention curves for the training materials, measured otherwise than by transfer, are falling, retention as measured by the transfer method does not decline. Whatever of the training the subject uses in the test has not been lost during the intervening time. It is a plausible hypothesis that the transfer to the second problem has been chiefly in terms of general factors, such as modes of attack, which are presumably more resistant to forgetting than are specific items. On the other hand, Hamilton's (1950) results indicate that one type of non-specific transfer (transfer of set to perform) occurs over only short periods of time. This, in fact, is one of the reasons for believing that transfer of set is different from other forms of transfer.

There are many problems connected with the retention of transfer effects which need to be worked out. In particular, the relationship between time and transfer needs to be investigated over a considerably wider range of learning situations.[25]

GENERAL INTERPRETATIONS OF TRANSFER

There can be no doubt that transfer, both positive and negative, occurs. Beyond the very first period of life, transfer determines, in part, the rate of learning of each new activity which the individual

[25] The results on this problem with animal subjects are worth mentioning. Ho (1928) obtained no consistent variation of transfer with time interval, thus corroborating the results obtained on human subjects. Bunch and Rogers (1936) find transfer to be greatest when 1 and 7 days separate the training and test mazes, least when a zero interval and 14 days separate them. When training is to partial mastery of a maze, transfer is greater after 2 days than immediately, and from that point to 120 days there is virtually as much transfer as immediately (Bunch and Lang, 1939; cf. also Bunch, 1941). Using a second habit which is antagonistic to the first, transfer in terms of trials is negative when the second habit is learned immediately and after 2 and 7 days, but in terms of both measures becomes positive when the learning of the second habit is after 12 and 28 days (Bunch, 1939).

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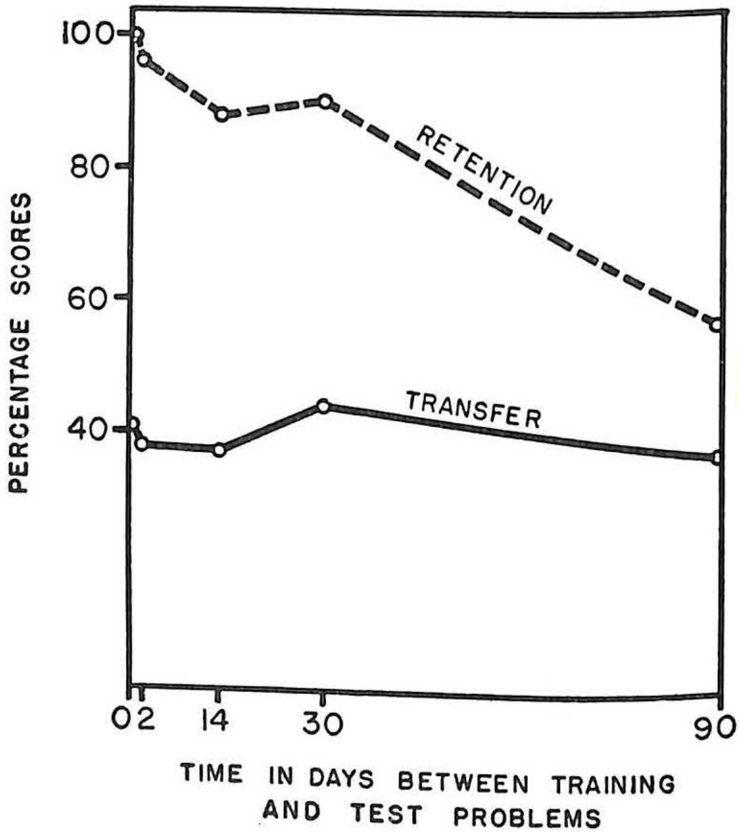

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acquires. It occurs in the form of stimulus generalization, that a learned response, once connected with a particular stimulus condition, will also be elicited by other, similar stimuli. It occurs, also, in the form summarized by the law of response generalization, that a stimulating condition, once connected with a particular response, will also elicit other responses which are related in some way to the first one. It appears in the learning of a single task, in the learning of successive tasks of the same sort, and between the learning of different types of tasks. It may also appear in the form of transfer of general factors such as general principles, sets, and modes of attack which may be common to the activities involved.

Given the facts of transfer, how are they to be formulated in a sufficiently general way to unify them and to permit prediction? Theories of transfer are chiefly theories about what is retained from training and used to facilitate or inhibit learning in new situations. They differ, thus, from theories of forgetting, which formulate the conditions under which retention fails in test situations which are often very little different from those of original learning. On the other hand, the findings on transfer are intimately related to the findings on forgetting as a function of altered stimulating conditions. It will also be noted (Chapter X) that negative transfer of training may be considered as one of the fundamental conditions of the forgetting process.[26]

A Theory of Transfer by Identical Elements

The only theory of identical elements to arouse widespread attention and discussion is the one formulated by Thorndike (1914). It asserts that training in one activity influences another activity

[26] Not all theories of transfer will be covered in this section. The point of view representing the doctrine of formal discipline, together with the faculty theory of mind on which it has often been made to rest, fails so completely of experimental support that there is no need to discuss it. Discussions of the doctrine of formal discipline will be found in Thorndike (1914) and Sandiford (1929). The possibility that training transfers by virtue of stimulating growth has been examined in the experiments of Gates and Taylor (1925, 1926), has not been supported, and, in any case, lacks generality. An article by Woodrow (1939), reporting that subjects acquired no increased general verbal ability as a result of practice at verbal tests, is interesting in this connection.

only insofar as the two have elements or aspects in common. Thus, training in addition transfers to multiplication because addition is necessary to multiplication—that is, is identical with one phase of multiplication—plus the fact that other events, such as eye-movements and resistance to other stimuli outside the problem, are common to the two activities.

For Thorndike, all learning consists in the modification of connections between situations and responses, but these two terms may vary from great specificity to great generality. Connections may be between such relatively simple terms as one word and another, as in the learning of vocabulary, or they may involve very abstract or subtle or general situations, which may be identical elements in two otherwise different situations. The essential fact is the identicality. In view of the inclusive range of events which Thorndike regards as "elements," the critics' charges of atomism are difficult to understand. Thorndike's elements may often be amorphous, but they are not "atoms" in any ordinary meaning of that metaphor. They seem to mean any clearly discriminable aspect of two activities which is the same in each.[27]

This theory has been severely criticized, but more often at the level of the systematic point of view which it is taken to represent than at the level of the experimental data (cf. Orata, 1928; Cook, 1936; Allport, 1937). It has also been widely accepted. If the theory holds that transfer is a function of identities only, it is fragmentary and inadequate, although it will fit some of the data. Thorndike uses the word, "identity," but he writes as if he intended the theory to cover more than strict identity.[28]

[27] There are, in one sense, no identities; there are only objects and events which may be classified in the same way. No event is ever repeated identically. Experimentally, however, stimuli and responses may be called "identical" when they are not discriminable by an independent set of operations.

[28] Although Guthrie (1935, p. 183) virtually denies that a general theory of transfer is possible, it is interesting to note that his general theory fits very well with an identical elements point of view. It is possible to explain transfer of training in Guthrie's terms by asserting that amount of transfer will depend upon the number of "conditioners" in common between the two learning tasks. This position would be consistent with Guthrie's point of view concerning other matters such as forgetting or his explanation of the gradual improvement in performance with practice.

A Theory of Transfer by Generalization

It can be said with assurance that transfer is a function of the relations between the already learned activities and the subsequent activities which are in the process of being learned. The experimental work already described has laid the foundations for the ordering of these relations along certain fundamental dimensions. Stimulus generalization diminishes as the test stimuli are moved away from the training stimuli along dimensions of pitch or loudness (Hovland, 1937a, b) or spatial distance (E. J. Gibson, 1939). Analogous equivalence of stimulation varies with both the kind and amount of alteration in the pattern of visual stimulation (McKinney, 1933) and is a function of similarity of pattern (Leeper and Leeper, 1932; Wolfle, 1935). Transfer of a naming response from one stimulus to another is a function of both degree and locus of identity of spelling of the stimulus syllables (Yum, 1931). In analogous experiments it also varies with the degree of resemblance or similarity, increasing as similarity increases (Yum, 1931; Gulliksen, 1932; E. J. Gibson, 1941). Thus, it seems safe to say that the tendency of an individual to make a previously learned response in a new learning situation depends upon the similarity between the stimuli in the two tasks. The occurrence of a previously learned response may be appropriate or inappropriate in the new learning situation, however. Under circumstances where such responses are appropriate, positive transfer will occur. Where these responses are not appropriate, negative transfer will result.

It is probable, also, that amount of response generalization is a function of the amount of similarity between the responses in the training and test activities. Thus, if both the responses and the stimuli involved are highly similar, positive, transfer is likely to occur. Response generalization depends, in part, however, upon the nature of measurement in the learning situation.

Wylie (1919) and Bruce (1933) have stated this general thesis, but in terms of the identity of stimuli and responses. Thus, we have two situations, the first determining positive transfer and the second determining negative transfer. In both, the terms A and B are first

connected with each other. When a new stimulus term is to be connected with the old response term, positive transfer results; when the old stimulus term is to be connected with a new response term (this will be recognized as the Müller-Schumann paradigm for associative inhibition) negative transfer results. These relationships are diagrammed below:

The extension of these paradigms to include "similar" stimuli and responses may be attributed to the empirical findings on stimulus and response generalization and to the theoretical work of Spence (1937), E. J. Gibson (1940, 1941), Osgood (1949), and Gagné, Baker, and Foster (1950), among others.[29]

It is to be recognized that other variables than stimulus and response similarity play a role in determining transfer of training. Amount of original learning, time between training and test, the nature of the task to be learned, and other conditions of practice will also partially determine amount of transfer in any given instance. The exact mode of operation of these and other variables, however, cannot be stated with precision at the present time.

The net transfer which our measurements give us is seldom the

[29] Because they are concerned with only special groups of transfer phenomena, because an adequate exposition of their logic requires more space than can be given them here, and because they derive primarily from work with animals, Hull's miniature systems will be no more than mentioned. These systems are important in their own right, and also because they have served as models for much of the theoretical work which is mentioned above. In his systematic attempt to account for stimulus equivalence, Hull (1939) includes partial physical identity of the stimuli as one of his basic mechanisms and thus overlaps a theory of identical elements. His concept of the habit-family hierarchy (1934) covers response equivalence, and his paper on the mechanisms of the assembly of behavior segments is of high importance for a theory of transfer (1935). Further work may demonstrate that these systems can embrace wide ranges of data from human subjects, and, as they stand, they represent a serious attempt to give a scientific explanation of many transfer phenomena. Advanced students of learning should be thoroughly acquainted with these papers.

result of a single factor. This is particularly true in complex learning situations where transfer is apt to be a residuum after positive and negative factors have been balanced. The net result may be positive transfer in spite of the fact that the retained effects of the first activity clearly interfere with parts of the second activity. Thus, using as the test activity a code in which stimulus items were identical with those in the training series and, therefore, designed to produce interference, Siipola (1941) finds a net positive transfer effect, but at the same time, large numbers of reversion errors—that is, movements in the direction learned in the training series. The negative influence of these errors, however, has been outweighed by the action of positive factors, presumably general ones of method and set.

The Generality of Transfer

It has been necessary to disregard, thus far, the wider relations of the facts and to write about transfer as if it were an isolated phenomenon. No datum, however, stands alone; all are interrelated to make up the complex universe of learning. The systematic relations of transfer to other categories of learning are so pervasive and important that some of them should be made explicit here.

After small amounts of learning early in the life of the individual, every instance of learning is a function of the already existent learned organization of the subject; that is, all learning is influenced by transfer. A few illustrations will suggest its pervasiveness. The initial status of an individual in any activity he is beginning is a composite of positive and negative transfer effects, and his status for some time thereafter may be heavily influenced by these effects. He has never practiced the act before, but nevertheless he does not begin at zero, because the act demands many responses he already has in his repertoire. Consider a college student who has never typed a line in his life and who now begins practice at typing. Although he has had zero direct practice, he has had a vast amount of indirect and applicable practice. He knows the alphabet, how to understand the instructions given to him, how to make the requisite coordinations of individual fingers, how to read and spell (in the

majority of cases), and much else which he needs to know in order to type and which he must now only coordinate into a smoothly functioning series of responses.

The influence of transfer is especially great in determining rate of learning as a function of the character of the material, as a function of age, and as a function of sex. It does not occur only from one activity to another, but appears at every step in successive trials within a single activity. The responses at successive trials produce patterns of stimuli not before received in exactly that form. These differences are usually smaller than those between activities, but they are still considerable. As a result of progressive response to changing stimulus situations because of prior learning on the preceding trials, a learning curve is also a curve of cumulative transfer. This is a further specification of the statement made in Chapter I, that a learning curve is, in part, a curve of cumulative retention.

The learning of complex, abstract, meaningful materials and the solution of problems by means of ideas (reasoning) are to a great extent functions of transfer. Where the subject "sees into" the fundamental relations of a problem or has "insight," transfer seems to be a major contributing condition. It is, likewise, a basic factor in originality, the original and creative person having, among other things, unusual sensitivity to the applicability of the already known to new problem situations. Perceiving, at whatever level, is probably never free of the influence of transfer. In a word, there is no complex psychological function or event which is not in some way a function of transfer of training.

REFERENCES

Allport, G. W. *Personality: a psychological interpretation.* New York: Holt, 1937.

Ammons, R. B. Acquisition of motor skill. I. Quantitative analysis and theoretical formulation. *Psychol. Rev.*, 1947, 53, 263-281.

Ammons, R. B. Acquisition of motor skill. II. Rotary pursuit performance with continuous practice before and after a single rest. *J. exp. Psychol.*, 1947, 37, 393-411.

Archer, C. P. Transfer of training in spelling. *Univ. Ia. Stud. Educ.*, 1930, 5, No. 5.

Army Air Forces Aviation Psychology Program Research Reports. Reports No. *1–19*. Washington, D. C.: U. S. Government Printing Office, 1947-1948.

Bagley, W. C. *The educative process.* New York: Macmillan, 1905.

Bair, J. H. The practice curve: A study in the formation of habits. *Psychol. Monogr.*, 1902, 5, No. 19.

Baker, K. E., and Wylie, R. C. Transfer of verbal training to a motor task. *J. exp. Psychol.*, 1950, 40, 632-638.

Barlow, M. C. Transfer of training in reasoning. *J. educ. Psychol.*, 1937, 28, 122-128.

Bass, M. J., and Hull, C. L. The irradiation of a tactile conditioned reflex in man. *J. comp. Psychol.*, 1934, 17, 47-65.

Behrens, H. D. Effects of a "how to study" course. *J. Higher Educ.*, 1935, 6, 195-202.

Bell, H. M. Rest pauses in motor learning as related to Snoddy's hypothesis of mental growth. *Psychol. Monogr.*, 1942, 54, No. 1.

Bills, A. G. *General experimental psychology.* New York: Longmans, Green, 1934.

Bingham, H. C. Size and form perception in *gallus domesticus. J. Anim. Behav.*, 1913, 3, 65-113.

Bray, C. W. Transfer of training. *J. exp. Psychol.*, 1928, 11, 443-467.

Bruce, R. W. Conditions of transfer of training. *J. exp. Psychol.*, 1933, 16, 343-361.

Bunch, M. E. The amount of transfer in rational learning as a function of time. *J. comp. Psychol.*, 1936, 22, 325-337.

Bunch, M. E. Transfer of training in the mastery of an antagonistic habit after varying intervals of time. *J. comp. Psychol.*, 1939, 28, 189-200.

Bunch, M. E. A comparison of retention and transfer of training from similar material after relatively long intervals of time. *J. comp. Psychol.*, 1941, 32, 217-231.

Bunch, M. E., and Lang, E. S. The amount of transfer of training from partial learning after varying intervals of time. *J. comp. Psychol.*, 1939, 27, 449-459.

Bunch, M. E., and McCraven, V. G. The temporal course of transfer in the learning of memory material. *J. comp. Psychol.*, 1938, 25, 481-496.

Bunch, M. E., and Rogers, M. The relationship between transfer and the length of the interval separating the mastery of the two problems. *J. comp. Psychol.*, 1936, 21, 37-52.

Carr, H. A. *Psychology: a study of mental activity.* New York: Longmans, Green, 1925.

Cook, T. W. Studies in cross education. I. Mirror tracing the star-shaped maze. *J. exp. Psychol.*, 1933, 16, 144-160.

Cook, T. W. Studies in cross education. V. Theoretical. *Psychol. Rev.*, 1936, 43, 149-178.

Coover, J. E. Formal discipline from the standpoint of experimental psychology. *Psychol. Monogr.*, 1916, 20, No. 87.

Coppock, H., and Mowrer, O. H. Inter-trial responses as "rehearsal": a study of "overt thinking" in animals. *Amer. J. Psychol.*, 1947, 60, 608-616.

Cox, J. W. Some experiments on formal training in the acquisition of skill. *Brit. J. Psychol.*, 1933, 24, 67-87.

Coxe, W. W. The influence of Latin on the spelling of English words. *J. educ. Res.*, 1924, 9, 223-233.

Crafts, L. W. Routine and varying practice as preparation for adjustment to a new situation. *Arch. Psychol., N. Y.*, 1927, 14, No. 91.

Culler, A. J. Interference and adaptability: an experimental study of their

relation with special reference to individual differences. *Arch. Psychol., N. Y.*, 1912, 3, No. 24.

Dallenbach, K. M. The effect of practice upon visual apprehension in school children, Parts I and II. *J. educ. Psychol.*, 1914, 5, 321-334; 387-404.

Dashiell, J. F. Some transfer factors in maze learning by the white rat. *Psychobiol.*, 1920, 2, 329-350.

Dunlap, K., and Snyder, A. Practice effects in intelligence tests. *J. exp. Psychol.*, 1920, 3, 396-403.

Fernberger, S. W. The effects of practice in its initial stages in lifted weight experiments and its bearing upon anthropometric measurements. *Amer. J. Psychol.*, 1916, 27, 261-272.

Fisher, R. A. *The design of experiments.* London: Oliver and Boyd, 1935.

Franz, S. I. Studies in cerebral function. VIII. Training in touch perception and cross-education. *Univ. Calif., Los Angeles, Publ. Educ., Philos, Psychol.*, 1933, 1, 121-128.

Franz, S. I., and Layman, J. D. Studies in cerebral function. I. Peripheral retinal training and practice transfer. *Univ. Calif., Los Angeles, Publ. Educ., Philos, Psychol.*, 1933, 1, 65-78.

Gagné, R. M., Baker, K. E., and Foster, H. On the relation between similarity and transfer of training in the learning of discriminative motor tasks. *Psychol. Rev.*, 1950, 57, 67-79.

Gagné, R. M., and Foster, H. Transfer of training from practice on components in a motor skill. *J. exp. Psychol.*, 1949, 39, 47-68.

Gagné, R. M., Foster, H., and Crowley, M. E. The measurement of transfer of training. *Psychol. Bull.*, 1948, 45, 97-130.

Gates, A. I., and Taylor, G. A. An experimental study of the nature of improvement resulting from practice in a mental function. *J. educ. Psychol.*, 1925, 16, 583-592.

Gayton, A. H. The discrimination of relative and absolute stimuli by albino rats. *J. comp. Psychol.*, 1927, 7, 93-105.

Gibson, E. J. Sensory generalization with voluntary reactions. *J. exp. Psychol.*, 1939, 24, 237-253.

Gibson, E. J. A systematic application of the concepts of generalization and differentiation to verbal learning. *Psychol. Rev.*, 1940, 47, 196-229.

Gibson, E. J. Retroactive inhibition as a function of degree of generalization between tasks. *J. exp. Psychol.*, 1941, 28, 93-115.

Gibson, J. J. Adaptation, after-effect and contrast in the perception of curved lines. *J. exp. Psychol.*, 1933, 16, 1-31.

Gibson, J. J., and Hudson, L. Bilateral transfer of the conditioned knee-jerk. *J. exp. Psychol.*, 1935, 18, 774-783.

Gibson, J. J., Jack, E. G., and Raffel, G. Bilateral transfer of the conditioned response in the human subject. *J. exp. Psychol.*, 1932, 15, 416-421.

Gulliksen, H. Transfer of response in human subjects. *J. exp. Psychol.*, 1932, 15, 496-516.

Gulliksen, H., and Wolfle, D. L. A theory of learning and transfer. II. *Psychometrika*, 1939, 3, 225-251.

Gundlach, R. H., and Herington, G. B., Jr. The problem of relative and absolute transfer of discrimination. *J. comp. Psychol.*, 1933, 16, 199-206.

Guthrie, E. R. *The psychology of learning.* New York: Harper, 1935.

Guthrie, E. R., and Horton, G. P. *Cats in a puzzle box.* New York: Rinehart, 1946.

Hamilton, C. E. The relationship between length of interval separating two learning tasks and performance on the second task. *J. exp. Psychol.*, 1950, 40, 613-621.

Hamilton, R. J. Retroactive facilitation as a function of degree of similarity between tasks. *J. exp. Psychol.*, 1943, 32, 363-376.

Harlow, H. F. The formation of learning sets. *Psychol. Rev.*, 1949, 56, 51-65.

Helson, H. Insight in the white rat. *J. exp. Psychol.*, 1927, 10, 378-396.

Hendrickson, G., and Schroeder, W. H. Transfer of training in learning to hit a submerged target. *J. educ. Psychol.*, 1941, 32, 205-213.

Hilgard, E. R., and Marquis, D. G. *Conditioning and learning.* New York: Appleton-Century, 1940.

Ho, Y. H. Transfer and degree of integration. *J. comp. Psychol.*, 1928, 8, 87-99.

Hovland, C. I. The generalization of conditioned responses. I. The sensory generalization of conditioned responses with varying frequencies of tone. *J. gen. Psychol.*, 1937a, 17, 125-148.

Hovland, C. I. The generalization of conditioned responses. II. The sensory generalization of conditioned responses with varying intensities of tone. *J. genet. Psychol.*, 1937b, 51, 279-291.

Hovland, C. I. The generalization of conditioned responses. IV. The effects of varying amounts of reinforcement upon the degree of generalization of conditioned responses. *J. exp. Psychol.*, 1937c, 21, 261-276.

Hulin, W. S., and Katz, D. Transfer of training in reading Braille. *Amer. J. Psychol.*, 1934, 46, 627-631.

Hull, C. L. Simple trial-and-error learning: a study in psychological theory. *Psychol. Rev.*, 1930, 37, 241-256.

Hull, C. L. The concept of the habit-family hierarchy and maze learning: Parts I and II. *Psychol. Rev.*, 1934, 41, 33-54; 134-152.

Hull, C. L. The mechanism of the assembly of behavior segments in novel combinations suitable for problem solution. *Psychol. Rev.*, 1935, 42, 219-245.

Hull, C. L. The problem of stimulus equivalence in behavior theory. *Psychol. Rev.*, 1939, 46, 9-30.

Hull, C. L. *Principles of behavior.* New York: Appleton-Century, 1943.

Hunter, W. S. Habit interference in the white rat and in human subjects. *J. comp. Psychol.*, 1922, 2, 29-59.

Irion, A. L. The relation of "set" to retention. *Psychol. Rev.*, 1948, 55, 336-341.

Irion, A. L. Reminiscence in pursuit-rotor learning as a function of length of rest and of amount of pre-rest practice. *J. exp. Psychol.*, 1949, 39, 492-499.

Irion, A. L., and Wham, D. S. Recovery from retention loss as a function of amount of pre-recall warming up. *J. exp. Psychol.*, 1951, 41, 242-246.

Jackson, T. A. General factors in transfer of training in the white rat. *Genet. Psychol. Monogr.*, 1932, 11, 1-57.

Jackson, T. A., and Jerome, E. A. Studies in the transposition of learning by children. VI. Simultaneous vs. successive presentation of the stimuli to bright and dull children. *J. exp. Psychol.*, 1943, 33, 431-439.

Jackson, T. A., Stonex, E., Lane, E., and Dominguez, K. Studies in the transposition of learning by children. I. Relative vs. absolute response as a function of amount of training. *J. exp. Psychol.*, 1938, 23, 578-600.

James, W. *The principles of psychology.* New York: Holt, 1890. Vol. I.

Jones, H. E., and Dunn, D. The configural factor in children's learning. *J. genet. Psychol.*, 1932, 41, 3-13.

Judd, C. H. The relation of special training to general intelligence. *Educ. Rev.*, 1908, 36, 28-42.

Katona, G. *Organizing and memorizing: Studies in the psychology of learning and teaching*. New York: Columbia Univ. Press, 1940.

Kellogg, W. N., and Walker, E. L. "Ambiguous conditioning," a phenomenon of bilateral transfer. *J. comp. Psychol.*, 1938, 26, 63-77.

Kline, L. W. Some experimental evidence in regard to formal discipline. *J. educ. Psychol.*, 1914, 5, 259-266.

Kline, L. W. An experimental study of associative inhibition. *J. exp. Psychol.*, 1921, 4, 270-299.

Klüver, H. *Behavior mechanisms in monkeys* (with an introduction by K. S. Lashley). Chicago: Univ. Chicago Press, 1933.

Köhler, W. Optische Untersuchungen am Schimpansen und am Haushuhn. *Abh. Preuss. Akad. Wiss., Phys-math. Kl.*, 1915, No. 3.

Kuenne, M. R. Experimental investigation of the relation of language to transposition behavior in young children. *J. exp. Psychol.*, 1946, 36, 471-490.

Langer, W. C. An investigation of the positive transfer effects in the learning of a sensori-motor task. *J. Psychol.*, 1937, 3, 371-379.

Leeper, R., and Leeper, D. O. An experimental study of equivalent stimulation in human learning. *J. gen. Psychol.*, 1932, 6, 344-375.

Leuba, J. H., and Hyde, W. An experiment in learning to make hand movements. *Psychol. Rev.*, 1905, 12, 351-369.

Lewin, K. Das Problem der Willensmessung und das Grundgesetz der Assoziation. I, II. *Psychol. Forsch.*, 1921, 1, 191-302; 1922, 2, 65-140.

Lewis, D., Shephard, A. H., and Adams, J. A. Evidences of associative interference in psychomotor performance. *Science*, 1949, 110, 271-273.

Lewis, M. H. Elemental versus configural response in the chick. *J. exp. Psychol.*, 1930, 13, 61-75.

Luchins, A. S. Mechanization in problem solving: the effect of Einstellung. *Psychol. Monogr.*, 1942, 54, No. 248.

Ludgate, K. E. The effect of manual guidance upon maze learning. *Psychol. Monogr.*, 1923, 33, No. 148.

Martin, M. A. The transfer effects of practice in cancellation tests. *Arch. Psychol., N. Y.*, 1915, 4, No. 32.

Martin, P. R., and Fernberger, S. W. Improvement in memory span. *Amer. J. Psychol.*, 1929, 41, 91-94.

McClelland, D. C. Studies in serial verbal discrimination learning. IV. Habit reversal after two degrees of learning. *J. exp. Psychol.*, 1943, 33, 457-470.

McGeoch, J. A., and Oberschelp, V. J. The influence of length of problem and of transfer upon rational learning and its retention. *J. gen. Psychol.*, 1930, 4, 154-168.

McKinney, F. Quantitative and qualitative essential elements of transfer. *J. exp. Psychol.*, 1933, 16, 854-864.

Melton, A. W. The methodology of experimental studies of human learning and retention. I. The functions of a methodology and the available criteria for evaluating different experimental methods. *Psychol. Bull.*, 1936, 33, 305-394.

Melton, A. W. Review of Katona, *Organizing and memorizing*. *Amer. J. Psychol.*, 1941, 54, 455-457.

Melton, A. W., and Irwin, J. McQ. The influence of degree of interpolated learning on retroactive inhibition and the overt transfer of specific responses. *Amer. J. Psychol.*, 1940, 53, 173-203.

Melton, A. W., and Von Lackum, W. J. Retroactive and proactive inhibition in retention: evidence for a two-factor theory of retroactive inhibition. *Amer. J. Psychol.*, 1941, 54, 157-173.

Meredith, G. P. Consciousness of method as a means of transfer of training. *Forum Educ.*, 1927, 5, 37-45.

Morgan, R. L., and Underwood, B. J. Proactive inhibition as a function of response similarity. *J. exp. Psychol.*, 1950, 40, 592-603.

Mudge, E. L. Transfer of training in chemistry. *Johns Hopk. Stud. Educ.*, 1939, No. 26.

Mukherjee, K. C. The duration of cutaneous sensation (I) and the improvement of its sensible discrimination by practice (II). *J. exp. Psychol.*, 1933, 16, 339-342.

Müller, G. E., and Schumann, F. Experimentelle Beiträge zur Untersuchung des Gedächtnisses. *Z. Psychol.*, 1894, 6, 81-190; 257-279.

Munn, N. L. Bilateral transfer for learning. *J. exp. Psychol.*, 1932, 15, 343-353.

Münsterberg, H. *Beiträge zur experimentellen Psychologie.* Freiburg: Mohr, 1889. Heft 4.

Norcross, W. H. Experiments on transfer of training. *J. comp. Psychol.*, 1921, 1, 317-363.

Orata, P. T. The theory of identical elements, being a critique of Thorndike's theory of identical elements and a re-interpretation of the problem of transfer of training. Ph.D. Dissertation. Columbus: Ohio State Univ. Press, 1928.

Osgood, C. E. Meaningful similarity and interference in learning. *J. exp. Psychol.*, 1946, 36, 227-301.

Osgood, C. E. The similarity paradox in human learning: a resolution. *Psychol. Rev.*, 1949, 56, 132-143.

Perkins, F. T., and Wheeler, R. H. Configurational learning in the goldfish. *Comp. Psychol. Monogr.*, 1930, 7, No. 31.

Perry, H. M. The relative efficiency of actual and "imaginary" practice in five selected tasks. *Arch. Psychol., N. Y.*, 1939, No. 243.

Peterson, J. C. The higher mental processes in learning *Psychol. Monogr.*, 1920, 28, No 129.

Pressey, L. C. The permanent effects of training in methods of study on college success. *Sch. and Soc.*, 1928, 28, 402-404.

Razran, G. H. S. A quantitative study of meaning by a conditioned salivary technique (semantic conditioning). *Science*, 1939, 90, 89-90.

Razran, G. H. S. Studies in configural conditioning. V. Generalization and transposition. *J. genet. Psychol.*, 1940, 56, 3-11.

Razran, G. H. S. Some psychological factors in the generalization of salivary conditioning to verbal stimuli. *Amer. J. Psychol.*, 1949a, 62, 247-256.

Razran, G. H. S. Sentential and propositional generalizations of salivary conditioning to verbal stimuli. *Science*, 1949b, 109, 447-448.

Reed, H. B. A repetition of Ebert and Meumann's practice experiment on memory. *J. exp. Psychol.*, 1917, 2, 315-346.

Robinson, E. S. *Association theory today; an essay in systematic psychology.* New York: Century, 1932.

Rubin-Rabson, G. Studies in the psychology of memorizing piano music. VI. A comparison of two forms of mental rehearsal and keyboard overlearning. *J. educ. Psychol.*, 1941, 32, 593-602.

Ruger, H. A. The psychology of efficiency: an experimental study of the processes involved in the solution of mechanical puzzles and in the acquisition of skill in their manipulation. *Arch. Psychol., N. Y.*, 1910, 2, No. 15.

Sackett, R. S. The influence of symbolic rehearsal upon the retention of a maze habit. *J. gen. Psychol.*, 1934, 10, 376-395.

Sackett, R. S. The relationship between amount of symbolic rehearsal and retention of a maze habit. *J. gen. Psychol.*, 1935, 13, 113-128.

Salisbury, R. A study of the transfer effects of training in logical organization. *J. educ. Res.*, 1934, 28, 241-254.

Sanderson, S. Intention in motor learning. *J. exp. Psychol.*, 1929, 12, 463-489.

Sandiford, P. *Educational psychology; an objective study*. New York: Longmans, Green, 1929.

Schwarz, G. Über Rückfälligkeit bei Umgewöhnung. I. Teil. Rückfalltendenz und Verwechslungsgefahr. *Psychol. Forsch.*, 1927, 9, 86-158.

Sears, R. R., and Hovland, C. I. Experiments on motor conflict. II. Determination of mode of resolution by comparative strengths of conflicting responses. *J. exp. Psychol.*, 1941, 28, 280-286.

Shipley, W. C. An apparent transfer of conditioning. *J. gen. Psychol.*, 1933, 8, 382-390.

Siipola, E. M. A group study of some effects of preparatory set. *Psychol. Monogr.*, 1934, 46, No. 210.

Siipola, E. M. Implicit or partial reversion-errors: a technique of measurement and its relation to other measures of transfer. *J. exp. Psychol.*, 1940, 26, 53-73.

Siipola, E. M. The relation of transfer to similarity in habit-structure. *J. exp. Psychol.*, 1941, 28, 233-261.

Siipola, E. M., and Israel, H. E. Habit-interference as dependent upon stage of training. *Amer. J. Psychol.*, 1933, 45, 205-227.

Sleight, W. G. Memory and formal training. *Brit. J. Psychol.*, 1911, 4, 386-457.

Spence, K. W. Analysis of the formation of visual discrimination habits in chimpanzees. *J. comp. Psychol.*, 1937a, 23, 77-100.

Spence, K. W. The differential response in animals to stimuli varying within a single dimension. *Psychol. Rev.*, 1937b, 44, 430-444.

Spence, K. W. Failure of transposition in size-discrimination of chimpanzees. *Amer. J. Psychol.*, 1941, 54, 223-229.

Starch, D. Transfer of training in arithmetical operations. *J. educ. Psychol.*, 1911, 2, 306-310.

Stroop, J. R. Studies of interference in serial verbal reactions. *J. exp. Psychol.*, 1935, 18, 643-662.

Stroud, J. B. Experiments on learning in school situations. *Psychol. Bull.*, 1940, 37, 777-807.

Thorndike, E. L. The effect of practice in the case of a purely intellectual function. *Amer. J. Psychol.*, 1908, 19, 374-384.

Thorndike, E. L. *The psychology of learning*. New York: Teachers College, 1914.

Thorndike, E. L. Practice effects in intelligence tests. *J. exp. Psychol.*, 1922, 5, 101-107.

Thorndike, E. L. A note on assimilation and interference. *Amer. J. Psychol.*, 1937, 49, 671-676.

Thorndike, E. L., and Woodworth, R. S. The influence of improvement in one mental function upon the efficiency of other functions. (I.); II. The estimation of magnitudes; III. Functions involving attention, observation, and discrimination. *Psychol. Rev.*, 1901, 8, 247-261; 384-395; 553-564.

Thune, L. E. The effect of different types of preliminary activities on subsequent learning of paired-associate material. *J. exp. Psychol.*, 1950a, 40, 423-438.

haus curve of retention for nonsense syllables, obtained by the saving method after he was almost certainly at a practice level,[1] falls rapidly from the completion of learning to the first point of measurement (19 minutes), after which time only 58.2 per cent of the original learning time is saved. In negative terms, this means that 41.8 per cent has been forgotten. From this point onward, the curve falls at a decelerated rate until, after 31 days, there is a saving of 21.1 per cent or a forgetting of 78.9 per cent.

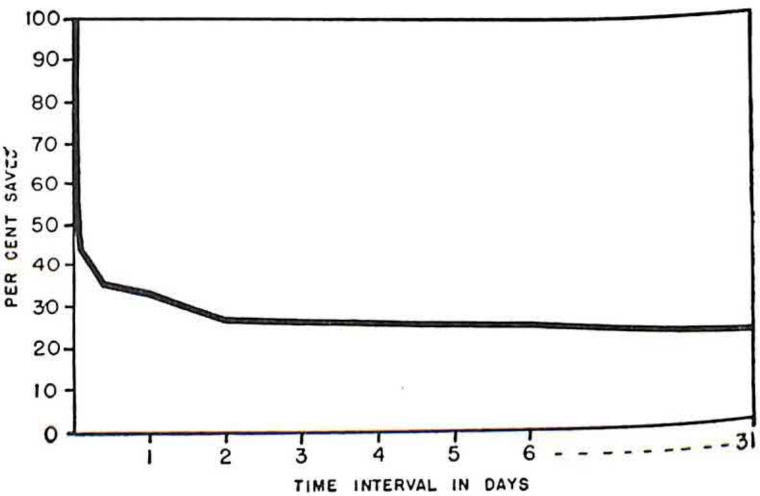

FIG. 37. EBBINGHAUS' CURVE OF RETENTION BY THE SAVING METHOD
(From data of Ebbinghaus, *Memory*, p. 76)

Most curves of retention for nonsense syllables, measured by relearning and saving, agree in showing a negatively accelerated form. The fact that many of them agree, not only in the form of the curve, but also in the percentages of saving, is the more important in view of the differences in experimental conditions which have existed.[2] It may be concluded that, over a wide range of conditions, the course of retention for nonsense syllables may be repre-

[1] It will be recalled that Ebbinghaus used but a single subject, himself.

[2] Cf. Finkenbinder (1913), Luh (1922), and Van Ormer (1932). Even under different conditions of measurement, such as the use of recall or reconstruction, the general form of the curve is often obtained (cf. Luh, 1922; and Bean, 1912).

sented by a curve which has its most rapid fall during the time immediately after the cessation of practice and which declines more and more slowly with increasing interval.[3]

The retention curves for the verbatim memory of meaningful materials may run at different heights from those of nonsense syllables, but most of them fall with relative rapidity during the period immediately after the end of practice and at a declining rate thereafter. The decelerating curve in Figure 38, which runs at a relatively

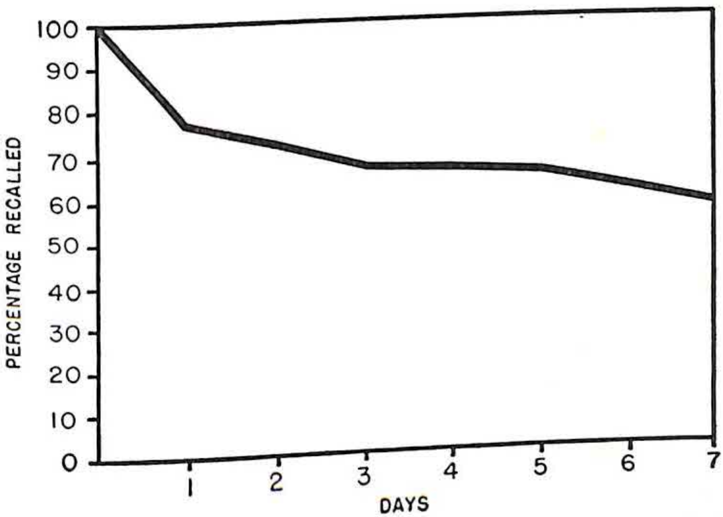

FIG. 38. A RETENTION CURVE FOR 50 MONOSYLLABIC WORDS, AS MEASURED BY RECALL

(From Williams, *J. exp. Psychol.*, 1926, 9, p. 373)

high level of retention, was plotted by Williams (1926) from the records of adults who studied lists of 50 monosyllabic words for 5 minutes, then recalled them immediately and after an interval. A different group of subjects was used at each interval. The immediate

[3] The reminiscence phenomenon must be regarded as an exception to this generalization. Reminiscence was not obtained in the studies listed above, probably because all of these experiments employed a fairly long interval between learning and the first measurement of retention. Reminiscence is quite transitory in this type of learning situation (Ward, 1937) and, had it occurred in these experiments it would have dissipated before the first retention measure was made.

recall score was taken as the 100 per cent point and the delayed recalls expressed as per cents of the immediate recalls. Retention curves for poetry and for prose material seem to follow the same general course. Similarly, the curve obtained for memory of content or substance seems to show a negatively accelerated curve (Dietze and Jones, 1931) as shown in Figure 39. This general curve form was also obtained by Briggs and Reed (1943). Stroud (1940) has reviewed the work on retention of school subjects and concludes that a negatively accelerated retention curve is most commonly

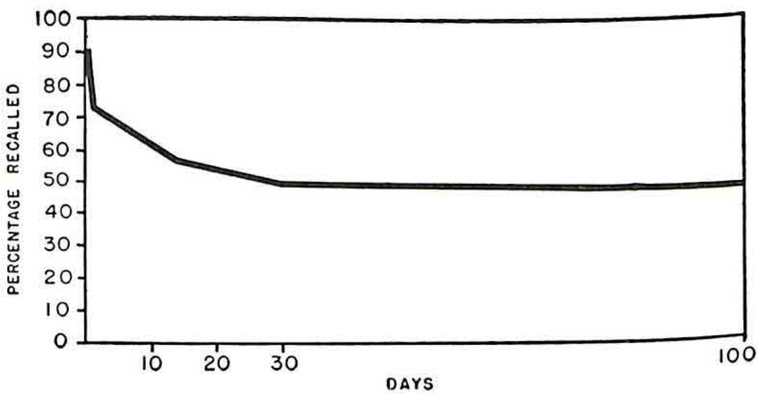

FIG. 39. A RETENTION CURVE (RECALL) FOR THE CONTENT OF FACTUAL PROSE READ BUT ONCE

(From Dietze and Jones, *J. educ. Psychol.*, 1931, 22, p. 670)

Only the intervals after which measurements were made are listed on the abscissa.

found. From an inspection of the curves in Figures 37, 38, and 39, it will be seen that, as the meaningfulness of the material increases, rate of forgetting becomes slower and the lower asymptote of the retention curve appears to rise. This is probably a valid conclusion concerning retention, though no systematic study of this relationship has been accomplished.[4]

Perceptual-motor acts vary quite widely in the degree to which they are subject to verbal control. The retention of some perceptual-

[4] This conclusion, of course, applies only to verbal learning. The retention value of perceptual-motor acts is very high and conditioned responses show little decline over long periods of time if extinction trials are not given. (Hilgard and Campbell, 1936).

motor skills, then, will reflect the retention of verbal habits. In other cases, however, verbal retention probably plays a very minor role. In general, the retention of perceptual-motor habits is quite high. The most systematic study in this field is that of Tsai (1924) upon the retention of a stylus maze habit. The maze was learned by massed practice to a criterion of 3 successive, perfect trials and was

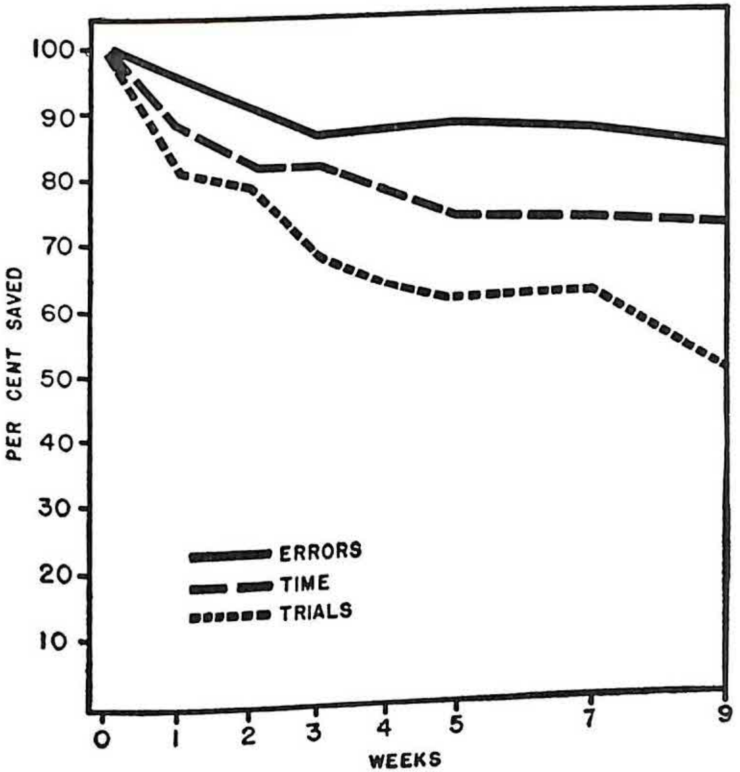

Fig. 40. Retention Curves for a Stylus Maze Habit
(From Tsai, *Comp. Psychol. Monogr.*, 1924, 2, p. 6)

relearned to the same criterion after intervals of from 1 to 9 weeks by different groups of subjects at each interval. The saving scores for trials, time, and errors are shown in Figure 40. This study suffers from the fact that maze habits are quite subject to verbal control. Nevertheless, the conclusion to be drawn from Tsai's data, that the retention value of perceptual-motor habits is high, is warranted.

important factor. It can hardly have been the major condition of the other experimental results cited.

Allowing in all of these cases for some incidental rehearsal during the retention intervals, and recognizing that in many cases the experiments were not designed to measure long-time retention and that, therefore, the later measurements were made under less well-controlled conditions, we must still conclude that under many circumstances there may be measurable retention over periods as long as several years, and perhaps as long as 46 years. Although experimental evidence is largely lacking for the longer intervals of time, this conclusion would receive rich support from clinical findings.

Retention under Abnormal Conditions

The clinical literature contains numerous references to temporary hypermnesia—that is, to heightened recall of experiences which had occurred a considerable time before. Hypermnesia occurs in manic states, in moments of danger, while going under an anaesthetic, and in some states of delirium. The clinical findings imply an even longer retention than the experimental measurements show.

Stratton (1919) has described a number of cases of what he calls retroactive hypermnesia, in which the subject has clear recall of stretches of experience during the hours immediately preceding some shock, usually an emotionally exciting one. Many of the details thus reinstated are of the kind not ordinarily recalled at all. These cases are important here, not only because the recall takes place after a long interval, but also because things commonly perceived, but not recalled in the meantime, are affected. The implication is that much of experience leaves an effect on the organism and may be recalled (at least under special conditions), although it occurs but for a moment and in the meantime has not been rehearsed. This suggests that the limits of the learning and retention of experience may be much more remote than is often supposed.

Under hypnosis there may be a heightened recall, both of past formal learning and of everyday experience (Stalnaker and Riddle, 1932; Hull, 1933) and even of recently learned meaningful poetry

(White, Fox, and Harris, 1940). Similarly, Pascal (1949) finds that a relaxation technique employing waking suggestion brings about greater retention. Psychoanalysts assert that the analytic use of free association can elicit the recall of experiences which date back to very early childhood and which the subject has not reinstated for many years. Under normal conditions of recall, items may be elicited which go back to the second, third, and fourth years of life (Gordon, 1928; Dudycha and Dudycha, 1933, 1941).

There is small question that the temporal limits of retention are much higher than the experimental measurements made with word lists and similar materials imply. Much that is perceived but once and seldom, if ever, reinstated thereafter may be retained and may, under the proper conditions, be recalled.

This raises the question of whether forgetting is ever complete—whether, that is, the limit of retention is the life span of the individual. It would certainly seem that many retention curves become asymptotic to values greater than zero. The long retention of early experiences, the heightened recall under abnormal conditions, and the clinical evidence concerning the recovery of early experiences under psychoanalytic treatment all point to the relative permanence of much that we learn. It may be, also, that there are more general effects of practice, such as the building up of generalized modes of response, transfer effects to other activities, and the like, which are retained indefinitely. It is, therefore, probably safest to conclude with James (1890) that "nothing we ever do is, in strict scientific literalness, wiped out."

Qualitative Changes in Retained Activities

Since the days of Ebbinghaus, the dominant concern of studies of retention has been the objective measurement of amounts retained and the influence of controlled conditions upon them. There are, however, qualitative changes in retention which tend to become overlooked in the quantitative studies. Not only is less recalled as time goes on, but what is recalled becomes changed or distorted. Early studies of the qualitative aspects of retention were concerned

with the memory image. Other qualitative events were reported, but through the protocols of introspection ran the dominant thread of the image. The problem has become wider than this, however, and includes such matters as the changes which take place with repeated and serial reproduction of material.

Certain reportable events often accompany the process of remembering. The usual method of studying these is to present the subject with a series of items, such as forms or words, and, after varying intervals, to ask for reproduction or recognition. This is followed by a request for a report on the subjective events accompanying the responses elicited. The introspective protocols reveal a context of reported material, much of which has no apparent connection with the materials learned. These reported events may be useful mnemonic devices employed by the subject or they may be the results of transfer of training, that is, free associations resulting from the subject's individual past experience. At any rate, they vary from subject to subject and from condition to condition within a single subject, and a judgment can seldom be formed of the extent to which they act as determiners of the objective responses on a test of retention.

The events reported are images, mainly visual and auditory; sensory context, such as kinaesthetic strains; and verbal context, usually associated in some way with the experimental situation. Less often reported, but almost certainly often present, is a set or attitude which gives direction to the reported events. Feelings of familiarity or unfamiliarity, of recognition or the lack of it, are often present regardless of the method by which retention is being measured. Each reported event may be tinged, also, with pleasantness or unpleasantness.

Kuhlmann's (1906) work on the reproduction of meaningless visual forms is an illustration of the complex interplay of difficulty of reproduction and the qualitative events which appear. If the form could readily be reproduced, a visual image might not be recalled in advance, though parts might be visualized during the construction of the drawing. The subject might draw more difficult forms as well as he could from visual imagery and then resort to recognition

to decide upon the degree of correctness. Verbal associations sometimes preceded and aided in eliciting imagery; sometimes the imagery elicited the words. More recently, Jenkin (1935) has found words to be the chief medium for the recall of visual figures by both children and adults, and visual imagery to be a distinctly secondary device.

There is one class of imagery, called *eidetic imagery*, which has a clarity approaching that of perception. Eidetic imagery is largely confined to children, and has been little studied in the context of other experiments on retention. It stands, however, as an interesting form of retention, usually immediate. The subject is shown a complex picture for a brief period (usually less than a minute), after which he recalls it in some detail, apparently by projecting it on a gray mat or other surface and observing the projected image. The eidetic subject may recall more completely than does the noneidetic subject, although he does not always do so.[7]

Philippe (1897) made an investigation, now become classical, of the alterations in qualitative retention with the passage of time. The learning materials were five objects, tactually presented, and retention was tested by reproduction in drawings. With time, details dropped out one after another, until finally, individuality was lost. At the same time the drawings became more generalized and approached a type representing an object apparently better known or more familiar than the original. These conclusions have been extended by a series of later investigations. Crosland (1921), using such materials as pictures and drawings, concluded that, in addition to the complete failure to recall anything, forgetting illustrates the operation of two reciprocal processes. The first process is disintegration and loss of detail, with which goes a typification of the type described by Philippe. The second process is assimilation, a progressive selection and addition, whereby the reproduction becomes different from the original. It is interesting that the changes undergone in this academic and intellectualistic experiment would resemble the Freudian dynamisms of condensation displacement, dramatization, and secondary elaboration.

[7] References to work on eidetic imagery will be found in Woodworth (1938).

Over the past twenty years, papers have been accumulating upon the qualitative changes in patterns of lines which are drawn successively. Much of the work has been devoted to the gestalt interpretations of these changes and, in particular, to their statement by Wulf (1922). For the present purpose, however, the results will be summarized without reference to theory. These experiments have in common the presentation of patterns of lines at which the subjects are instructed to look carefully with intent to learn. Presentation is for relatively short times, e.g., 1.5 to 20 seconds. Reproduction is called for soon after exposure and again after varying intervals. The characteristics of such reproductions are almost impossible to quantify, and the conclusions of such research must, of necessity, rest upon the interpretation of the experimenter.

The character of the specific changes in reproduced figures is almost certainly a function of the original figures, the instructions to the subjects, the method and time of presentation, the characteristics of the subjects, and a number of other variables. In spite of the complex conditions which are operating in this situation, however, the results show certain uniformities. Unfamiliar or less familiar figures tend to be assimilated toward more familiar forms. The character of the changes is a function of verbal analysis, and the particular verbal analysis which is made will necessarily be determined by the prior training and the existent verbal organization of the subject. There is also a frequent trend toward increasing symmetry and simplicity (cf. Fig. 41).[8]

These conclusions freeze in much too stereotyped a form the manifold variations which reproduced figures undergo. No two reproductions are identical, and upon each one there has been a complex play of perceptual and reproductive determinants. The results of these studies, however, stand as a vigorous warning that the quantitative data which the usual experiments yield do not tell the whole story of retention.

[8] Characteristic references to the work on which this section draws are Wulf (1922), Gibson (1929), Allport (1930), Perkins (1932), W. Brown (1935), Hall (1936), Hanawalt (1937), Burton and Tueller (1941), and Goldmeier (1941). A more detailed discussion of these and related data will be found in Woodworth (1938).

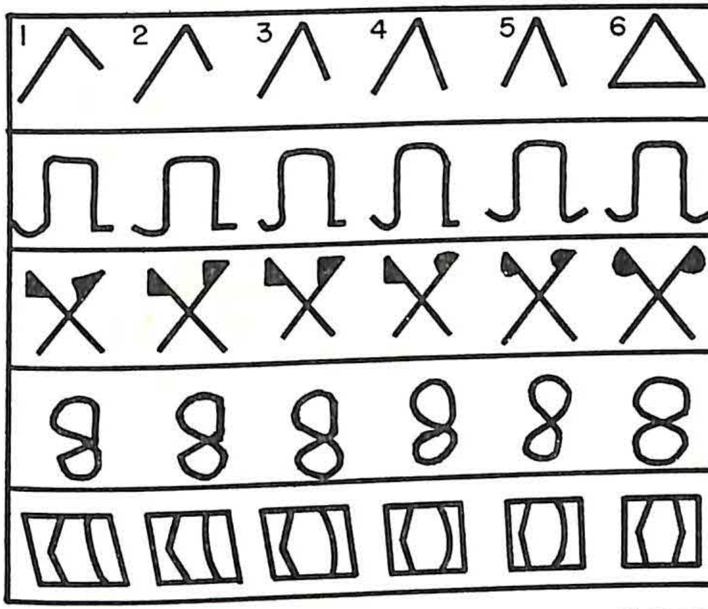

FIG. 41. EXAMPLES OF SUCCESSIVE REPRODUCTIONS OF FIGURES
(From Perkins, *Amer. J. Psychol.*, 1932, 44, p. 482)

Changes in Reproduced Verbal Materials

Much of the material learned and retained in the course of daily life is difficult to analyze into a series of measurable units. Progressive changes in this material are, therefore, difficult to chart. Meaningful materials are subject to innumerable variations in specific wording, in verbal sequence, in shades of meaning, and the like, which are so complex as to defy the usual detailed analysis. The best that can be done, on the basis of our present knowledge, is to frame classifications which seem to cover large groups of data.

The studies of the psychology of testimony (*Aussage-psychologie*) [9] have characteristically used materials, such as pictures, events, collections of objects, which come close to the common materials

[9] A large sampling of the papers in the field of *Aussage-psychologie* may be found in Stern's journal, *Beiträge zur Psychologie der Aussage*, 1903-1906, which was largely devoted to this problem. Whipple's *Manual* (1921) contains references to other bibliographies and to reviews of the literature. Only a small amount of work in this field has been published since 1921.

of everyday life. In these studies, the material is presented but once for a brief time, whereupon the subject is asked to write a narrative or to answer a list of questions on it. After the lapse of an interval, he is tested again. Experimenters in this field obtain certain qualitative results with sufficient frequency to narrow sharply the inevitable penumbra of subjectivity.

What is observed and fixated in the first place is a function of the motivation and set of the subject, and this initial set gives direction to the qualitative course of retention through time. Other sets which may be operating at the time of later recall in turn exercise their influence until, as a result of these factors and the other complex determinants of recall, the material as recalled may only partially resemble in from the material originally presented. Reports are often changed to make the event or material reported conform more closely to what the subject considers the fitting or to-be-expected occurrence. Descriptions of persons approach some familiar type; unperceived details are filled in to make the narrative more "rational" or more acceptable to the subject; and emphases are shifted according to similar principles. What the subject takes to be the major features are often elaborated and worked over, changes are made in the interests of better formal expression, and, in manifold other ways, the original is assimilated to the organization of the subject and reworked in terms of it. As in the recall of figures, we find the trend from the less familiar to the more familiar, the altering influence of verbal analysis, and the drift toward a kind of symmetry.

A related and more systematic study of qualitative changes with time has been made by Bartlett (1932), whose book, *Remembering*, in which the major experiments are reported, is notable for its recognition and documentation of the interrelations among perception, learning, retention, and social psychology. The pattern of his experimental procedure follows the experiments already mentioned. Meaningful materials, such as pictures, stories, or picture-writing, were studied by the subjects and recalled, usually after several different time intervals. As in the case of the *Aussage* material, an exact recall is the exception rather than the rule. Once the first

recall has been made, the pattern or outline of the material tends to persist, but the detailed features vary greatly. The successive recalls show a reworking and reduction of the material to a form which can be readily and satisfyingly dealt with by the subject. Pictorial materials approach more conventional representations, are rendered more familiar and recognizable. Those which are readily recognizable in the first place undergo simplification which may be followed, after several successive reproductions, by elaboration into a form which only remotely resembles the original.

In the reproduction of connected verbal materials (stories), unfamiliar details either drop out or tend to be changed into more familiar words. With frequent reproduction, certain items early become stereotyped, changing little thereafter, while the processes of omission of detail, simplification, and modification toward the familiar may go on as long as unaided recall is possible.[10]

The results of one series of experiments by the *method of serial reproduction*, while only secondarily relevant to this section, are important for their bearing upon the social diffusion of information, the transmission of cultural materials, and the spread of rumor. This method differs from the others in that A's reproduction becomes the learning material for B, whose reproduction becomes the material for C, and so on. The repeated reproduction, in this case, is from person to person, rather than from time to time by the same person. Such serial reproduction is marked by great alterations in the material. Although the subjects believe themselves to have reproduced the important features with virtually no change, omitting only the unessential, the actual variation from the original is great. Specific items, such as names and numbers, seldom remain accurate through more than a few reproductions; opinions and conclusions may be changed to the opposite meaning; and so on. It appears that each subject works over the material into a form which fits his own motives and criteria of suitability and reasonableness.

It is easy to see in these qualitative changes, as in those previously

[10] In a sense, all work on qualitative features of recall, particularly of repeated recall by the same subjects, is relevant here, but, for other work immediately relevant to Bartlett's, cf. Wees and Line (1937), Morris (1939), Clark (1940), and Tresselt and Spragg (1941).

described, the operation of two of the great classes of psychological events, motivation and transfer of training. What is recalled both immediately and after an interval is a function of the motivation of the subject, and of the influence of his existent organization of learned material. It is, thus, not the immediate material alone which determines recall, but its interaction with motivation and the retained residue of prior learning. When we add to this the fact that each person is in a state of constant variation, his ongoing activity changing from moment to moment, and when we consider that these materials are reproduced, always, in the context of the ongoing behavior of the subject, it is not difficult to understand the profound and complex qualitative changes which occur in retention.

RETENTION AS A FUNCTION OF THE CONDITIONS OF MEASUREMENT

A number of different measures of retention have been employed. Luh (1922), in a systematic study of the conditions of retention, determined, within a particular set of experimental conditions, the relationships pertaining among five measures of retention. These measures were: recognition, reconstruction, written anticipation (written recall), anticipation (recall), and relearning (saving). Other measures of retention have been employed in other situations: resistance to extinction, time for reaction evocation (latency), degree of transfer of training effect, and response amplitude being examples of such other measures. It should be recognized that the concept of retention possesses general meaning, as applied to these various measures, only to the extent that predictable relationships between these measures exist. At the present time, little information exists with reference to a determination of these relationships. The status of a general concept of retention as applied to indiscriminately to these measures rests largely upon the apparent similarities between the various measurement techniques and upon the faith of psychologists that certain relationships, as yet undetermined, do, in fact, pertain between the various measures called measures of retention.

The most thorough study of the form of retention curves as a

function of the method of measurement is the already mentioned experiment by Luh (1922). His subjects learned lists of 12 nonsense syllables by the anticipation method to a criterion of 1 perfect repetition. Retention was then measured after intervals of 20 minutes, 1 hour, 4 hours, 1 day, and 2 days.[11] The 5 curves (Fig. 42)

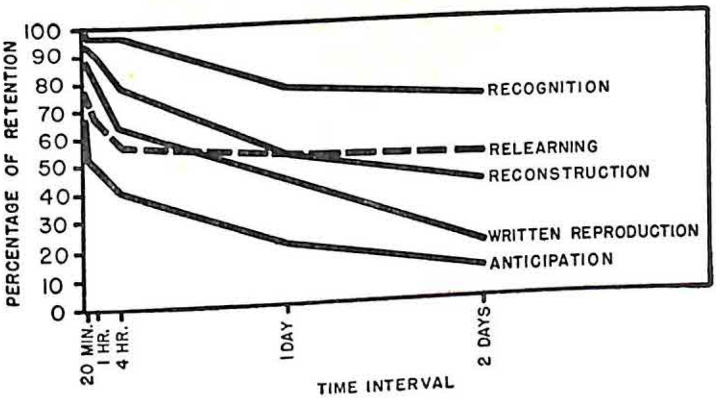

FIG. 42. RETENTION CURVES OBTAINED BY FIVE DIFFERENT METHODS OF MEASUREMENT

(From Luh, *Psychol. Monogr.*, 1922, 31, No. 142, p. 22)

are all negatively accelerated, but they have different slopes and may approach different limits. It will be noticed that the deceleration is slight in the curve for recognition and that the curve for the relearning method intersects those for written reproduction and reconstruction.

The deviation of the curve obtained by the relearning method from the curves yielded by the other methods makes desirable a statement about the characteristics of the relearning method. Relearning is a composite of two factors: (a) recall at the first relearn-

[11] In half of the lists learned by each, the subject was asked to anticipate at the first relearning presentation (recall), and immediately thereafter presentation was continued to the criterion (relearning). With the other half of the lists, the subject was instructed at the end of the retention interval to write as many of the items as could be recalled in five minutes (written reproduction). Immediately after this, he was given a list of 24 nonsense syllables from which he was to select the original 12 (recognition). Finally, he was asked to reconstruction the original order of the series (reconstruction). It must be remembered, in interpreting the results, that the three measures last mentioned were applied in sequence with the same subjects.

ing presentation and (b) subsequent learning. The two factors are inversely correlated, so that the greater the recall, the fewer will be the repetitions necessary for relearning. We shall also see that the relearning method is complicated by the warming-up phenomenon and that it is probably this latter factor which is responsible for the atypical shape of the retention curve as determined by the relearning method.[12]

The situation with respect to the measurement of retention is further complicated by the fact that the various measures of retention have been employed under a variety of general experimental procedures. The classical method of investigation used in studies of retention may be termed the *method of successive recalls.* Under this procedure, a single group of subjects learns to some criterion, after which the same group receives successive tests of retention at various time intervals following the completion of original learning. The early work of Ebbinghaus (1885) serves as an example of the application of this method. Also, as we have already noted (Chapter V), the early studies of reminiscence employed this general form of experimental design (cf. Ballard, 1913; Williams, 1926). Although many of the earlier studies of retention employed this method, it is not satisfactory for most of the purposes of quantitative measurement. Each succeeding measurement (other measures of retention besides recall could be used, of course) is a practice period, and the results are a composite of retention as a function of time (or the intervening activity) and of the intervening measurements of retention since original learning. The results obtained by Brown (1923), showing that, in recalling the names of the states of the United States, more names are recalled upon the second than upon the first attempt, forced a re-examination of this procedure. Later research has tended to use the *method of single recall,* wherein a group of subjects is used for the determination of each point on the retention curve.

The curves in Figure 43 illustrate the difference in results obtained under these two procedures. Hanawalt (1937) presented 8

[12] Other studies comparing measures of retention include Achilles (1920), Anastasi (1932), Clarke (1934), Stalnaker (1935), and Andrew and Bird (1938).

patterns of lines for periods of from 15 to 20 seconds, during which time the subjects were to examine and to draw the figures. They were again asked to reproduce the patterns either by the method of single recall or by the method of successive recalls at each of the time intervals indicated in the figure. Precautions were taken against possible rehearsal. With the method of successive recalls (successive reproductions, in this case) the same subjects drew

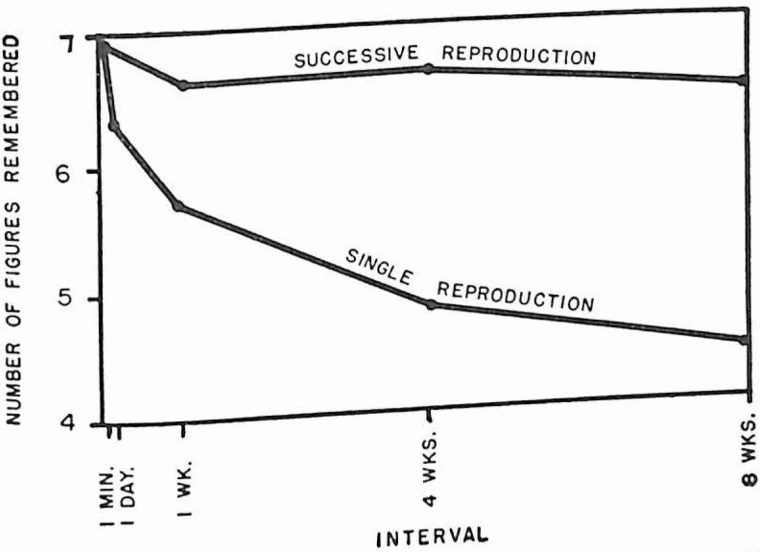

FIG. 43. RETENTION CURVES FOR NUMBER OF FIGURES RECALLED BY THE METHODS OF SINGLE REPRODUCTION AND SUCCESSIVE REPRODUCTION (From Hanawalt, *Arch. Psychol., N. Y.*, 1937, 31, p. 25)

the patterns after each successive interval, while with the method of single reproduction, different but equated groups were used. The curve for single reproduction has a negatively accelerated form and runs at a lower level than the curve for successive reproduction, which falls slightly for 1 week and not at all from that point to 8 weeks. Corroborating results have been published by K. B. Clark (1940) for meaningful prose.[13]

[13] One additional difficulty with the method of successive recalls is that the effect of the successive retention tests upon the retention curve is probably not independent of the time intervals chosen for study, since different degrees of distribution of practice are implied whenever different retention intervals are used.

the measurement of retention is longer than the time intervals between the successive trials of the learning period. Considered in this way, it is apparent that the popular view of this relationship is almost certainly incorrect. This view, held by many laymen and an occasional educator, is that a positive correlation exists between learning and forgetting. The bright student may acquire knowledge rapidly, but this learning is unstable. The dull fellow who must slave to acquire the most meager skill, on the other hand, is felt to have acquired a "sure" knowledge.

An experimental answer to this problem is complicated by differences in degrees of original learning as distinguished from the rate at which a given skill is reached and by a number of other conditions. If all subjects learn to the same criterion, and if retention is measured by relearning, the results may be influenced by a greater degree of learning of parts of the material by the slow learner, who has mastered some parts early in practice but is slow in mastering all, and by the fact that the rapid learners will also relearn rapidly. If, on the other hand, all subjects spend the same amount of time in practice, the slow learner will have learned less than the fast learner, and, hence, will have less to retain.[14]

The monograph by Gillette (1936) contains a critical discussion of experimental methods and a bibliography of work on this problem. The *method of adjusted learning* (originally devised by Woodworth) which he uses involves removing from the list at successive presentations items which have been responded to correctly. Using this method, Gillette has obtained clear evidence that the fast learner retains more of paired pictures and numbers than the slow learner, whether the measure be recall or relearning.

Similar results have been obtained by others, when differences in degree of original learning and other conditions have been fairly well controlled. This high positive relation between individual scores in learning and retention is to be expected, of course, from the fact that these are continuous processes. It seems, then, that the slow learner gains no advantage in retention from his slowness, and

[14] Underwood (1949b) discusses some of the technical problems involved in research on this and related problems. (p. 528ff.).

that the fast learner is at no disadvantage because of his superior speed. When comparisons are accurately made, no benign Emersonian compensation appears. What retentive superiority slow learners may seem to have in everyday life, insofar as it is a real and not a fancied superiority, may well derive from their greater overlearning of a small portion of the material.

Degree of Learning

The influence of degree of learning upon retention is sufficiently great to require consideration in nearly all experiments upon retention. One must remember, however, that degree of learning (experimentally considered) is not a single and unitary variable which has but one dimension. There are, instead, a number of different measures of degree of learning, and the one employed needs always to be specified. Number of trials of practice is one common measure. In perceptual-motor habits, time, errors, or degree of skill attained may be used. With verbal materials, number of correct anticipations or number of repetitions are the commonly used measures.

Using number of repetitions as his measure of degree of learning, Ebbinghaus (1885) attacked this problem with lists of 16 nonsense syllables practiced with frequencies varying from 0 to 64, followed after 24 hours by relearning. When saving is plotted against frequency, the curve is nearly linear (Fig. 44). When the results are expressed in terms of the mean saving in seconds for each repetition spent in learning, the mean is 12.7 with little variation around it. This figure, of course, would only apply for the particular retention interval employed in this experiment.

There are ample data to show that, over wide ranges of frequency and of material, increasing learning is followed by increasing retention, even when overlearning is involved. Krueger (1929), using lists of 12 monosyllabic nouns, has employed degrees of learning of 100, 150 and 200 per cent, where 100 per cent is taken as the number of trials to learn to one perfect recitation and where the two larger percentages mean that this number of trials was increased by half (150 per cent) and was doubled (200 per cent). Each of these

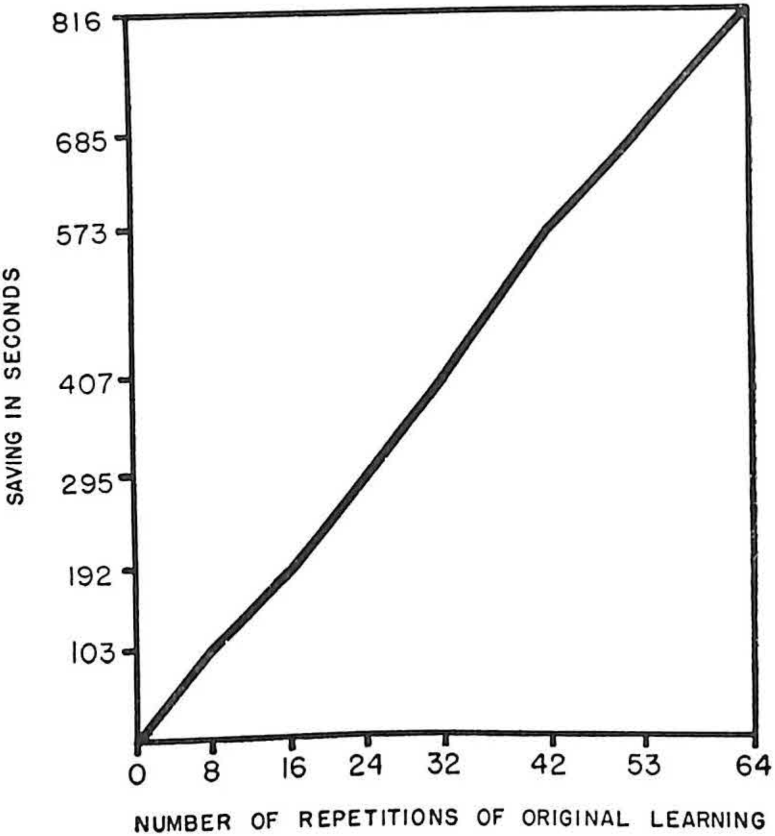

FIG. 44. A RELATION BETWEEN NUMBER OF REPETITIONS IN LEARNING AND SAVING AFTER 24 HOURS
(From data of Ebbinghaus, *Memory*, p. 56)

increments in frequency of repetition is accompanied by an increase in both recall and saving scores after each of the six intervals used, but the increase is smaller from 150 per cent to 200 per cent than from 100 per cent to 150 per cent (Table XXI). This diminishing return from high degrees of overlearning is in agreement with other results on the problem.

A similar study of the retention of finger mazes after intervals of 1, 2, 3, 4, 7, and 14 days gave similar results (Krueger, 1930). Both experiments show a uniformly positive relation between number of repetitions at original learning and the measures of retention

after each of the intervals used, with diminishing returns at the higher frequencies.

Other things being equal, retention increases as degree of original learning increases. This generalization emerges from studies with several kinds of material, when frequency of repetition is the indicator of degree of learning and when retention is measured by the recall and saving methods. The form of the function relating degree of learning and amount of retention varies with the conditions of experimentation. The curve may show initial acceleration when the retention interval is relatively long, but, whatever the initial form and the interval, the curve almost certainly becomes negatively accelerated at high degrees of learning. For retention over very long intervals, high degrees of learning are necessary. Such high degrees of learning are best obtained, in all likelihood, by means of distributed practice.

TABLE XXI

RECALL AND SAVING SCORES AS A FUNCTION OF DEGREE OF LEARNING

(From Kreuger, *J. exp. Psychol.*, 1929, 12, pp. 74 and 75)

Interval (Days)	Degree of Learning					
	100	150	200	100	150	200
	Mean Words Recalled			Saving Scores		
1	3.10	4.60	5.83	21.73	36.15	47.10
2	1.80	3.60	4.65	13.40	33.45	42.05
4	.50	2.05	3.30	3.40	29.75	32.30
7	.20	1.30	1.65	1.75	23.15	27.55
14	.15	.65	.90	1.65	20.80	25.45
28	.00	.25	.40	1.50	20.50	25.10

One such form of distribution consists of an original learning to a criterion, followed by repeated relearnings to the same criterion after a succession of intervals. When Ebbinghaus (1885) learned lists of syllables and stanzas of Byron's *Don Juan* to a criterion of one perfect reproduction and relearned them to this criterion after each of 5 successive intervals of 24 hours, he obtained the results shown in Table XXII. On each day the lists of syllables were brought to the same criterion, yet on each day after the first, relearning was

Retention as a Function of the Materials Learned

Even a casual inspection of the literature on retention will reveal that different materials have shown different amounts of retention. Many of the superficially justifiable comparisons between different materials cannot be made legitimately, however, because the conditions of original learning have not been comparable. If a determination of the relative retention values of different materials is to be made, three conditions must be fulfilled. First of all, a careful dimensional analysis of learning materials is needed before the problem of relative retention value can be stated adequately. In the second place, a comparison of retention values requires that all other conditions, such as degree of original learning, motivation, distribution of practice, and so on, be the same for the materials compared. This, however, is difficult to achieve and, in many cases, is impossible. It is obviously difficult to equate conditions for conditioned response learning, serial anticipation learning, delayed reaction learning, and the solution of *Umweg* and rational problems. Finally, comparable units of measurement must exist between the learning situations being compared. A score of 50 per cent time-on-target in the Koerth pursuit rotor situation does not mean the same thing as does the correct anticipation of 5 out of 10 nonsense syllables. Two trials in the conditioned galvanic skin response situation do not necessarily equal two trials in the rational learning situation. Time is a more satisfactory measure than trials, in some cases, and can sometimes be used with less equivocality than trials or frequency of correct response, but, considerably more often than not, this is not a feasible measure to use. This problem of obtaining comparable measures of learning between different situations has already been encountered in connection with our discussion of transfer of training (cf. Chap. IX). No satisfactory solution to this problem exists. Certain rough comparisons between materials can be made, however.

As we have noted, the study of perceptual-motor habits has often revealed that these skills are retained relatively well over considerable periods of time. Studies which make comparisons between

perceptual-motor skills and verbal habits, however, have uniformly been unable to achieve comparability. Furthermore, the distinction between perceptual-motor and verbal habits has sometimes been made as if a sharply defined dichotomy exists between them. Actually, almost no activities fall at the extremes of this continuum. Certainly, most of the activities employed in the laboratory do not. The stylus maze, for example, may be learned by various combinations of symbolic and motor processes, and there is no good reason to suppose that such problems as are involved in the mental maze involve considerable amounts of motor response.[16]

There is good ground for concluding that meaningful materials and those which involve the discovery and understanding of a principle are usually much better retained than are nonsense materials and others learned by rote. A few papers report data which imply that verbal materials with high association values are retained better than those with low, or that the more familiar, the more concrete, and probably the more meaningful are the better retained. Katona (1940) has published considerable data on the retention of learning which involves understanding. He concludes that the solution of a problem, achieved by discovery of a general principle, may be retained with little or no loss for considerable periods of time. Melton (1941) has pointed out the statistical invalidity of some of the data which support this conclusion. Nevertheless, the general conclusion is very probably sound. In view of the fact that meaningfulness, familiarity, and high association value all imply the transferability of previous learning, this is not surprising. Habits to which strong, previously learned habits can transfer should be retained better for a longer period of time than habits which receive no such support.

The role of intraserial interference as a determiner of the forgetting of different materials is illustrated by the fact that "crowded" materials are forgotten more rapidly than "isolated" materials

[16] The data that compare the retention values of motor and verbal habits do not yield uniform results. Papers by McGeoch and Melton (1929), Freeman and Abernethy (1930, 1932), McGeoch (1932), Waters and Poole (1933), Leavitt and Schlosberg (1944), and Van Dusen and Schlosberg (1948), may be consulted by readers who are interested in this problem.

(Buxton and Newman, 1940). In this experiment, the series to be learned contained six items of one kind (crowded material) and two nonadjacent items of another kind (isolated material). The greater retention of the isolated material points to a relative lack of intraserial interference as far as these items are concerned. This conclusion is supported by the data of Gibson (1942), which show that, as degree of intra-list generalization increases, retention value declines. Gibson employed paired-associate learning in which the stimuli were geometrical figures and the responses were nonsense syllables. The similarity of the geometrical figures could be varied in order to produce different amounts of intra-list generalization.

The Affective Tone of the Material

Pleasantness (P), unpleasantness (U), and indifference (I) are characteristics ascribed to experienced materials or activities, and the influence of affective tone on retention may be regarded as an aspect of the more general problem of retention as a function of the material. This sub-problem has been complicated by a lack of agreement among experimenters on the nature of affective tone, a lack of agreement which has appeared more often in their interpretations of results than in their experimental procedures. The latter have usually been to determine the affective tone of the materials by the judgments of the subjects. Whatever theory of feeling one accepts, it is necessary to take the experimental data at the level of the operations by which they were obtained and to consider the feeling tones studied as judged feeling. The chief materials and methods employed fall in certain broad classes.[17]

(a) By the *method of free recall of experiences of everyday life,* the subjects are asked to write down as much as they can recall of their experiences during a certain period and then to rate these

[17] The literature to 1929 on the general problem of the relationship between feeling tone and recall has been reviewed by Meltzer (1930) in a paper that discusses the methodological difficulties involved. Gilbert (1938) has reviewed the papers from that time through 1937. Additional work is reviewed in Edwards (1942). Extensive bibliography and discussion of the literature will also be found in Cason (1932) and Barrett (1938).

experiences as P, U, or I. This method is a hit-or-miss one, subject to influences from unknown variables, the temporary set and self-instruction of the subjects, and whether the subjects are rating the experiences for their affective value at the time of having them or at the time of rating them. It usually assumes, moreover, that the frequencies of P and U experiences are the same and that the percentages of recall may be taken as indicative of the retention values of the experiences thus toned.

The burden of proof rests on those who assume an equal frequency of P and U experiences, and proof of this is lacking. There is, on the contrary, considerable evidence against it. Flügel (1925) has published results from nine subjects who had kept a record of hedonically toned experiences for a period of thirty days. These records show a preponderance of P experiences over U. The same result appears in the records made by children on the day following a holiday (Wöhlgemuth, 1923) and it is probable, as Barrett (1938) suggests, that most subjects have been favorably disposed toward the events of their lives and are likely, thus, to have a preponderance of P experiences. When we add to this Cason's (1932) conclusion, that there is an "optimism of judgment" for affective tone, the method of free recall becomes questionable. Of course, it is possible to argue from these data that more P than U experiences are found because the subject remembers them better, even over the short periods of time involved in these experiments. This position, of course, leads to scientific meaninglessness.

The results obtained by this method are too complicated to interpret confidently. While some of the results show no difference between the numbers of P and U experiences recalled, the tendency of the evidence is toward the higher retention value of P experiences. The intensity of the affective tone seems more important, however, than its quality (cf. Edwards, 1942).

(b) A second method consists of *a pairing of an affectively toned item with a relatively neutral one*, as a P odor with a nonsense syllable or a number, and the measurement of retention after an interval. This method has been used relatively little. It faces the same difficulties as those to be described in connection with the

third method, but if interpretation is carefully made, it is a defensible procedure. In a characteristic experiment, Ratliff (1938) employed numbers of known difficulty paired (as second members) with P and U sounds, colors, and odors which had been carefully chosen and rated. Lists, learned to three correct repetitions, were recalled immediately and after both five and ten minutes. The differences between the recalls of numbers on presentation of P and U sensory cues do not have satisfactory reliability, but recall is uniformly greater and reaction time faster for responses to P colors and sounds. These relations are reversed, however, when olfactory stimuli are used.

(c) By a third method, *a large number of items, such as words, are rated as P, U, and I. Subjects learn lists constructed from the items judged to be in each class and, subsequently, recall, relearn, or try to recognize them after an elapsed interval.* This is the most frequently used, and probably the most important, of the three methods. In connection with this method, one must remember that items may not retain the same quality and intensity of affective tone when combined in lists or arranged in pairs. It is likewise unknown whether items judged to have certain affective characteristics in isolation will keep these attributes unchanged during successive presentations. The available data on affective habituation make it quite possible that they will not (Beebe-Center, 1932). Combination into lists is unavoidable, and the use of more than one repetition has been deemed necessary by most experimenters, but reports of subjects on the influence of combination and repetition would go far toward clarifying these issues.

A majority of the experiments which have compared the retention values of lists of words rated for affective tone have found retention to decrease in the order P-U-I. However, the differences have often been small, and there have been exceptions to the order noted above. Furthermore, in most cases, the experiments have been so inadequately designed as to permit of no valid conclusion. In different experiments, results on this problem have been shown to be a function of the set, attitude, or mood of the subject (Barrett, 1938), of the rated intensity of the affective tone (Barrett, 1938), of the

CA of the subjects (Gilbert, 1937), and probably of the time interval (cf. Gilbert, 1938). Set may operate as a selective factor, the particular set being aroused either by the stimulus to recall or by conditions independent of the material. Thus, a current irritability may condition selection and emphasis of U items and a current expectancy of good fortune may condition an emphasis on P items. If we accept the conclusion that, for most people, the sets, attitudes, and moods of life are more often P than U, the better retention of P material is to be expected on this basis.

Edwards' (1942) article advocates a restatement of the problem of the relation of P and U to learning and retention. According to the hypothesis he advances, the important variable to be considered is the frame of reference of the learner. Experiences which agree with this frame of reference are better retained than those which do not, regardless of the P or U characteristics of those experiences. Of course, on the whole, experiences which agree with one's frame of reference are judged to be P and those which do not are judged to be U. Edwards' (1941) paper on the retention of political views which are in or out of harmony with the learner's frame of reference lends support to this view.

Such a formulation of the problem is intimately related to the theory of feeling, which was initially advanced by Carr (1925), and has, since that time, received a considerable amount of research attention from Peters (cf., for example, 1935, 1938, 1939). This hypothesis, known as the judgmental theory of feeling, considers pleasantness and unpleasantness to be nothing more than a manifestation of the normal reaction tendency of the individual. Objects and situations toward which the individual has a normal reaction tendency of approach are judged to be pleasant, while those toward which the individual has a normal reaction tendency of avoidance are judged to be unpleasant. In these terms, we may conclude that those things toward which an individual has positive reaction tendencies tend to be remembered better than those toward which the individual has negative reaction tendencies or no reaction tendencies at all.

Many of the results in this field have been interpreted in terms

of the Freudian theory of repression of the unpleasant (1914, 1925), although most of the differences have been too small to support any hypothesis very strongly. An experiment by Sharp (1938), directed at testing the Freudian theory, has employed material sufficiently different from those already described to require mention here. It is her interpretation that Freud means by a pleasant experience one which is *acceptable*, in harmony with the individual's values, beliefs, and modes of conduct. By a similar token, an unpleasant experience is one which is *unacceptable*, out of harmony with the ideals, values, and modes of acting. It is apparent that these experiences are apt to elicit approach and avoidance responses, respectively. The continuity between this view and the positions taken by Edwards, Carr, and Peters is also fairly apparent.

The learning materials in this experiment, chosen because they were known to be acceptable, unacceptable, or neutral to the subjects, were taken from the case histories and clinical notes on the psychoneurotic and mildly psychotic subjects in the abnormal experimental group. They consisted of lists of 15 two-word items. Learning was to a criterion of one perfect repetition, with recalls after 2, 9, and 16 days, and a relearning after the last recall. Approximately equal numbers of normal subjects whose reactions to the items were not otherwise known also learned and recalled the lists. The groups which learned each class of words were roughly equivalent in CA and IQ.

The means and P.E.'s for learning and retention, given in Table XXIII, show a number of differences between the materials. From internal evidence, Sharp concludes that the differences in rate of learning are much more a function of hedonic differences than of the composition of the groups of subjects and that the retention scores are not primarily determined by the differences in degree of learning which probably accompanied the differences in rate. The unacceptable items are much more difficult to learn than are the neutral items and are retained less well, while the acceptable items are also more difficult to learn than are the neutral ones, but are retained better after the longer intervals. The acceptable and unacceptable items are learned at approximately the same rate, but

are markedly different in retention value, the acceptable ones being much the better retained.

When the recall scores are expressed in percentages of the total number of items and plotted, all curves show a sharp drop to 2 days, but the curve for the unacceptable items drops farther than either of the others. From that point onward, the unacceptable items show little change. The continued high values at 9 and 16 days are a function of the successive recalls, but successive recall cannot account for the differences between the materials. Interestingly enough, normal and abnormal groups show similar tendencies.[18]

Table XXIII

Mean Learning and Retention Scores for Three Classes of Items

(From Sharp, *J. exp. Psychol.*, 1938, 22, p. 407)

Subjects	N	Material (15 Items Each)	Trials to Learn	Recall 2 Days	Recall 9 Days	Recall 16 Days	Trials to Relearn 16 Days
No.	22	Ac.	$9.68 \pm .63$	$8.68 \pm .37$	$10.54 \pm .53$	$10.18 \pm .51$	$3.00 \pm .29$
No.	22	Nu.	$4.22 \pm .19$	$10.04 \pm .31$	$9.68 \pm .38$	$9.13 \pm .41$	$4.36 \pm .34$
No.	22	Un.	$8.77 \pm .42$	$6.45 \pm .39$	$5.95 \pm .40$	$5.72 \pm .33$	$6.90 \pm .41$
Ab.	23	Ac.	$9.86 \pm .82$	$9.17 \pm .34$	$9.60 \pm .40$	$9.17 \pm .38$	$3.56 \pm .26$
Ab.	23	Nu.	$6.52 \pm .65$	$9.56 \pm .28$	$8.56 \pm .26$	$7.69 \pm .36$	$4.39 \pm .25$
Ab.	23	Un.	$9.43 \pm .60$	$5.86 \pm .27$	$6.04 \pm .49$	$5.86 \pm .42$	$7.00 \pm .55$

No. = Normal. Ab. = Abnormal. Ac. = Acceptable. Nu. = Neutral. Un. = Unacceptable.

These results give a clear picture of higher retention value of acceptable material than of unacceptable, and of neutral material than of unacceptable, with neutral and acceptable not far apart, although the acceptable material is somewhat the better retained after the longer intervals. This appears also in the relearning scores after 16 days. The differences between acceptable and unacceptable materials are larger than those often found between P and U materials, which may be a function of the greater concreteness and personal significance of the items used by Sharp.

[18] Unpublished research by Heathers and Sears (reported in Sears, 1944) has failed to corroborate the findings of Sharp with normal subjects.

Uncompleted versus Completed Tasks

In 1927, Zeigarnik performed a group of experiments which became the starting point for a considerable amount of similar work. The procedure was to give the subjects a number of simple tasks at a continuous sitting, to permit them to finish half of the tasks, but to interrupt them on a plausible pretext before the other half had been completed. The conditions were counterbalanced to prevent the results from being a function of the character of the tasks themselves. At the end of the sitting, the subjects, who had not been told that interruption was an integral condition of the experiment, were asked to give the names of as many of the tasks as they could. The results showed a clearly greater recall of the names of the interrupted than of the completed tasks.[19] Results of a similar nature have since been obtained by a number of investigators.[20] The names of more uncompleted than of completed tasks also appear as reminisced items after 2 minutes, but after 2 days, 1 week, and 2 weeks more completed than uncompleted tasks are reminisced (Martin, 1940).

The conditions determining the greater recall of the names of the uncompleted tasks have not been studied in exhaustive detail, but enough is known about them to permit a statement of some of the more probable conditions. In part, the Zeigarnik effect is a function of "success" and "failure." With instructions which lead the subjects to believe that interruption means a successful performance and completion a poor performance, the I/C ratio (incomplete/complete) drops considerably below 1.00, thereby reversing the results found under the more usual conditions (Marrow, 1938). The motivational level of the subject is also a factor of importance. For example, Rosenzweig (1943) tested for the recall

[19] The means of the individual ratios (I/C) were 1.9 in one experiment, while the ratio computed from the means of the numbers of completed and uncompleted tasks was 1.6. Ratios of this magnitude have since been obtained by others.

[20] Failure to obtain such effects has also been reported (cf. Boguslavsky and Guthrie, 1941; Alper, 1946).

of completed and uncompleted puzzles under conditions of task orientation and ego orientation. In the former case, subjects were led to believe that the experimenter wished to determine facts about the puzzles while in the latter case, the subjects were instructed that the puzzles represented a test of their ability. This means, of course, that success and failure as well as completion and incompletion are factors which are involved in this experiment. Nevertheless, the results indicate that, under conditions of task orientation, the usual preponderance of incompleted tasks were recalled. Under conditions of ego orientation, however, more of the completed tasks were recalled. These results are supported by the findings of Glixman (1949) who used three degrees of "stress." In one case subjects were told that the experimenter was merely trying to determine the length of time required to perform each of the several tasks. In a second situation, subjects were told that the tests were an "Intellectual Alertness Inventory" on which norms were being obtained. In the third case, the subjects were informed that the test had already been standardized and that information was being obtained about individual ability with a hint being dropped that the student's academic future was involved in his performance. The results show that, as degree of stress increases, there is a decrement in the recall of the uncompleted tasks.

That individuals with different personality characteristics may react to the same objective situation in different ways to produce results similar to those listed above may be deduced from the findings of Rosenzweig and Mason (1934). In this case, the recall of completed and uncompleted tasks by children (CA 4 to 14 years) was analyzed. Those children who were rated as "proud" by their teachers tended to recall the completed tasks more than children who were not rated as possessing this personality characteristic in supranormal degree. Other personality characteristics may also be important. Subjects with greater neurocirculatory efficiency recall more interrupted tasks than do those with less, a result which Abel (1938) interprets in terms of personality characteristics, which, in turn, influence motivation. Rickers-Ovsiankina (1937) found that

normal subjects show a stronger tendency to complete a task once begun than do schizophrenics.[21]

Pachauri's (1935) correlation of 0.67 between average time allowed for performance of a task, whether completed or uncompleted, and frequency of recall, implies that the Zeigarnik phenomenon is a function of the length of the tasks. In the work of both Zeigarnik and Pachauri the dominance of the interrupted tasks grows less with time, but the form of this function and its conditions are unknown. It appears that, if a task is too difficult to be performed, or if it is very short, it derives no advantage at recall from having been interrupted. When, however, the difficulty of the tasks is in the middle range, the interrupted ones are retained better and recalled first. The I/C ratio is lowest at the first and last positions in the series of tasks and highest at the intermediate positions (Marrow, 1938).

It should also be noted that subjects not only recall the names of unfinished tasks more frequently, but they tend to resume them (Ovsiankina, 1928; Harrower, 1932). Related activities may, however, act as substitutes for the original one (Lissner, 1933). Statements concerning success or failure made by the experimenter at the point of interruption decrease the number of resumptions of the task, however (Nowlis, 1933). The interrelations of the results on the recall of the names of interrupted activities, and particularly the relations of Nowlis' results to the others, can be satisfactorily worked out only when the determining conditions are more adequately known.[22]

McKinney (1935) has studied retention as a function of interruption under conditions which are more characteristic of the usual laboratory experiments. Subjects who had been told that they were to learn a stylus maze to a criterion of three perfect trials in succession were stopped after they had made one perfect trial, and their retention after a week was compared with that of controls for whom

[21] Rosenzweig's (1935) paper should be consulted for a study of repetition of successful and unsuccessful activities as a function of age and personality (cf., also, Alper, 1946).

[22] Cf. three papers by Rethlingshafer (1941a, b, c) for a description of the specific behavior of subjects who have been interrupted, and for comparisons of behaviors of normal and feebleminded subjects.

one perfect trial was the criterion set by the instructions. Similar experiments were performed with interruption earlier in maze practice, with a retention interval of one day, and with verbal materials. The results of this extensive series of experiments are not wholly consistent, and the differences between conditions are seldom large, but the implications are that interruption is followed by a higher degree of retention if the interruption occurs before the activity has been brought to a level of performance which could be interpreted by the subjects as completion.

The better retention of the names of incomplete than of complete tasks, and the tendency to resume interrupted activities have been interpreted by Lewin (1935), Prentice (1944), and others in terms of quasi needs and tensions. The tension of a quasi need which has not been realized by fulfillment of the need may persist, finally to discharge in the form of recall or resumption when the occasion presents itself. If we omit the penumbra of gestalt theory which these concepts carry, they are much the same as the concept of motive. Stated in motivational terms, the results mean that, when a task which a subject is motivated to complete is interrupted before completion, thus leaving the motive unsatisfied, the task is likely to be resumed or recalled when the occasion permits. Rosenzweig (1941, 1943) has drawn the distinction between "need-persistive" and "ego-defensive" motives and reactions in an amplification of this general viewpoint and in an attempt to reconcile some of the irregularities of the data which occur in the Zeigarnik effect under conditions of stress or ego-involvement. This point of view, also, is more intimately related to psychoanalytic theory. According to it, under conditions of low stress, need-persistive reactions predominate, thus producing the increased recall and resumption of interrupted and unfinished activities. When, however, the incompletion of an activity becomes threatening to the individual, ego-defensive reactions tend to predominate. Consonant with the concept of the need-persistive reaction is the explanation offered by Freeman (1930) who, departing from measurements of muscular tension during completed and interrupted tasks, accounts for the resumption of the interrupted ones in terms of such concepts as competition, rein-

forcement, and inhibition. It is possible, also, that in many cases, the interruption may have caused the subjects to note the interrupted task more clearly, to wonder why it was interrupted, to estimate how much they had done, and, in general, to react to it in a way which made it more meaningful and enhanced its retention value.

PROACTIVE INHIBITION: THE EFFECTS OF PRIOR LEARNING

In the chapter on transfer of training (Chap. IX) it was noted that the phenomenon of proactive inhibition suffers from a certain ambiguity of definition. Whenever a previously established habit has a negative effect upon the establishment of a new habit, we may speak of proactive inhibition. Such a negative effect could also be termed negative transfer and could be studied under any of the experimental designs available for that purpose (Chap. IX). On the other hand, proactive inhibition has often been studied under a rather special form of experimental design which relates more closely to the designs for the study for retention and retroactive inhibition than to the designs for the study of transfer of training that we have considered. This latter type of design, according to the terminology developed by Melton, Waters, Underwood, and others, refers to the proactive inhibition of recall.[23] The experimental design in question is presented in schematic outline below:

Group I.	Rest	Learn A	Rest	Recall and Relearn A
Group II.	Learn B	Learn A	Rest	Recall and Relearn A

Following Underwood's terminology, we would refer to poorer learning of A in group II as an example of associative inhibition. Poorer recall and relearning of A in group II, however, would be an example of proactive inhibition. It goes without saying that the learning of A is carried out to the same criterion in both groups. This, however, raises an important practical problem. If the learning

[23] Underwood (1945) reserves the term "proactive inhibition" for the phenomena associated with this type of design and uses the term "associative inhibition" to refer to negative transfer produced under the designs discussed in Chapter IX.

of B in group II has any effect upon the subsequent learning of A, there is a distinct possibility that the two groups will not be equated with respect to the learning of A, even though learning is carried out to the same performance criterion in both groups. This is because the two groups will approach the criterial point at different rates and the subjects in the group with the faster rate will more often overshoot the criterion than will the subjects in the group with the slower rate (cf. Underwood, 1949b). Thus, the possibility exists that, if group II learns A more slowly than group I, the results in recall and relearning, far from being an example of proactive inhibition, might be an example of the fact that retention is higher following much learning than following little. On the other hand, if group II learns A more rapidly than group I, the results in recall and relearning will tend to underestimate the amount of proactive inhibition. The data which exist suggest that both types of effect (in the learning of A) may occur, depending upon such factors as the amount of prior learning of B, the time between the learning of B and the learning of A, and the criterion selected for the learning of A. While the methodological difficulty mentioned here probably has not contributed serious bias to the experimental results which have been obtained, it can be completely overcome by the addition of two more control groups, as outlined below:

Group I.	Rest	Learn A	No Rest	Recall and Relearn A
Group II.	Rest	Learn A	Rest	Recall and Relearn A
Group III.	Learn B	Learn A	No Rest	Recall and Relearn A
Group IV.	Learn B	Learn A	Rest	Recall and Relearn A

Under this plan, the existence of proactive inhibition would depend upon the demonstration that more decrement (or less gain) occurred in the recall of group IV as compared with group III than in the recall of group II as compared with the recall of group I. To date, no experiment has been conducted using such a design, and the lack of control introduced by the use of the two-group design may enter as a systematic error in the results which have been obtained. On the other hand, it is questionable that the increased precision offered by the four-group design is great enough to warrant the extra experimental labor involved.

A number of careful investigations of the proactive inhibition phenomenon have been conducted. From the results of these experiments, certain conclusions regarding the conditions of proactive inhibition can be drawn. These are summarized in the following paragraphs.

(A) *Proactive inhibition is a function of stimulus similarity.* If one of the determining conditions of proactive inhibition is transfer of training, our previous discussion of the conditions of negative transfer would lead us to expect that one of the most important conditions of proactive inhibition would be the similarity (formal or meaningful) of the stimuli involved in the two lists to be learned. This is corroborated by the results of a number of experiments (Blankenship and Whitely, 1941; Melton and Von Lackum, 1941; and, by inference, Bugelski, 1942; Underwood, 1944). The Melton and Von Lackum experiment may serve as an example of this type of finding. In this experiment, three proactive inhibition conditions were employed. One of these was a rest or control condition in which the subjects received 5 trials on a list of 10 consonant syllables (list A), rested 20 minutes, and then relearned the same list (A) to a criterion of two successive errorless trials. In the other two conditions, subjects received 5 trials of practice on a preliminary list (list B), then learned list A for 5 trials, rested 20 minutes and relearned list A. The difference between the two experimental conditions was that, in one case, list B was dissimilar to list A while, in the other case, lists A and B were made up from the same set of consonants and were, in consequence, similar. The results, as measured by recall scores taken from the first four relearning trials of list A, show a clear superiority of the rest condition, an intermediate position for the condition where A and B were dissimilar, and the greatest amount of proactive inhibition where A and B were similar. For example, on the first recall trial, the mean number of correct anticipations for these three conditions, respectively, was 2.08, 1.56, and 1.02. From this it may be seen that the percentage of proactive inhibition obtained was 25 per cent for the condition where A and B were dissimilar and 51 per cent for the condition where A and B were similar.

(B) *Proactive inhibition is a function of response similarity.* One of the implications of the theoretical transfer surface drawn by Osgood (1949) is that, with identical stimuli, increasing amounts of response similarity should result in less and less proactive inhibition and, finally, in proactive facilitation or positive transfer (see Fig. 34). This is supported by Osgood's (1946) finding that there is less interference in learning a second list if the responses are similar than if they are dissimilar, providing the stimuli are identical. More directly in the context of the proactive inhibition studies, the recent paper by Morgan and Underwood (1950) demonstrates the same thing. Basing their experiment on the unpublished findings of Haagen (1943) and Hughes (1949), Morgan and Underwood studied proactive inhibition under circumstances where the first and second lists of paired-associate adjectives had the same stimulus items. The response items, in the different conditions, bore five degrees of meaningful similarity to each other (between lists). The results demonstrate that proactive inhibition decreases in a fairly regular fashion as amount of response similarity increases.[24]

(C) *Time intervals in proactive inhibition.* A considerable number of experiments have been concerned with comparing the amounts of proactive and retroactive inhibition obtained under comparable circumstances. These studies have been oriented toward the theoretical position taken by Melton and Irwin (1940), which is discussed in another portion of this chapter. In general, it appears that the relative amounts of proactive and retroactive inhibition depend upon the time when the measurements are made. When only a short time interval separates the original learning from the recall and relearning of the relevant list, retroactive inhibition is consistently found to be greater than proactive inhibition (cf. Melton and Von Lackum, 1941; Underwood, 1945; McGeoch and Underwood, 1943). On the other hand, when a considerable time period, say forty-eight hours, separates original learning and recall, retroactive inhibition and proactive inhibition seem to occur in approximately equal amounts

[24] The findings on associative inhibition (original learning of the second list) in this experiment showed that facilitation occurred instead of inhibition, and that amount of this facilitation increased with increasing response similarity.

(Underwood, 1948a,b). This result appears to stem from an increase in the number of correct responses under conditions of retroactive inhibition and a decrease in the number of correct responses under conditions of proactive inhibition. The interpretation of this effect, however, we will delay until the Melton-Irwin theory is under discussion.

(D) *Proactive inhibition is a function of amount of preliminary learning.* Two studies bear directly upon this problem. The first, by Waters (1942), need be mentioned only in passing since he failed to obtain sufficient amounts of proactive inhibition to test this relationship adequately. The monograph by Underwood (1945), however, demonstrates a clear relationship between amount of proactive inhibition and the number of preliminary lists learned. In this experiment, subjects learned either 0, 2, 3, or 6 preliminary lists, then learned the significant list (i.e., the one to be proactively inhibited). After a rest of 25 minutes, this latter list was recalled and relearned. Proactive inhibition increased as the number of preliminary lists increased. This effect, however, was extremely transitory, being largely dissipated after the first relearning trial. One very interesting aspect of Underwood's findings in this experiment relates to the ease of the original learning of the significant list. As number of preliminary lists was increased, original learning of the significant list became increasingly facilitated. Thus, the condition which produced most proactive inhibition in the *recall* of the significant list also produced the greatest facilitation in the *learning* of that list. This finding suggests the operation of the warming-up effect which has been mentioned earlier in this book (especially in Chap. IX). According to this view, proactive inhibition represents a combination of positive warming-up effects and negative transfer effects. The amount of the warming-up effect should increase, at least up to a point, with the amount of preliminary learning (cf. Thune, 1950a, b; Irion, 1949b; Irion and Wham, 1951). The general effect of this warming up would, of course, be facilitative. Amount of negative transfer would also increase with amount of preliminary learning, but in this case the general effect would be inhibitory. The two effects probably tend to dissipate in time, but warming up dissipates

very much more rapidly than does negative transfer (cf. Hamilton, 1950; Bunch, 1936). It would also seem that transfer is very much more sensitive to the effects of stimulus and response similarity than is the warming-up effect. From such a point of view, certain deductions can be drawn.

(1) As amount of preliminary learning (or number of preliminary lists learned) increases there should be an increased facilitation of the learning of the significant list (at least up to an optimal point).

(2) With a given amount of preliminary training, amount of proactive inhibition should be a function of the time between the preliminary learning (and original learning) and the measurement of proactive inhibition. This relationship should be such that, if proactive inhibition is measured very soon after the preliminary and original learning, little or no proactive inhibition (or even proactive facilitation) should be obtained. With increasing time, however, increasing amounts of proactive inhibition should be obtained.

(3) Amount of proactive inhibition, measured after a constant time has elapsed since preliminary training, should be greater as the conditions fostering negative transfer are allowed to operate.

It is apparent that such a viewpoint will not explain all of the facts of proactive inhibition. It does, however, serve to consolidate some of the major findings.

THE CONDITIONS OF INTERPOLATION

The general decrement of retention curves is their most frequent characteristic. This decrement or forgetting begins in most cases soon after practice ceases and continues at rates which vary with the specific conditions already discussed. Even when the curve rises for a time after the close of practice (reminiscence), the decrement finally appears. Over and above the conditions of retention which have already been mentioned, there remains the question of the conditions which are most directly responsible for the occurrence of forgetting—the conditions which operate during the retention interval.

The fact of forgetting is almost as pervasive as the fact of learning itself. If retention during practice were complete, curves of learn-

ing would have forms different from those they so often have, distribution of practice might be little more effective than massed practice, serial position curves might not be as bowed, and many of the phenomena of learning would certainly be different from what they are. The appearance of a general decrement after periods of no practice is one of the haunting annoyances of life outside the laboratory. The fact known yesterday eludes us now; the history or mathematics of which we were master ten years ago must be relearned; the happenings of yesterday are recalled but poorly today.

Forgetting is a function of a number of major conditions. If we exclude the possibility that the major portion of forgetting may be explained by disuse or deterioration of the organic correlate of learning (see below), there are three such fundamental conditions: (a) interference by intervening activities; (b) altered stimulating conditions; and (c) inadequate set at the time of recall. Forgetting occurs because of these conditions and not as a matter of passive decay. A description of these three conditions, and of the circumstances upon which they, in turn, depend will constitute both a statement of the experimental facts and an elaboration and support of the theory that the three conditions listed provide an adequate behavioral explanation of forgetting.

The experimental problem is to measure the influence of each condition separately by holding two of them as constant as possible and varying the third. In daily life, however, all three conditions act together to determine forgetting; and even when two are controlled in the laboratory, the control is not complete. Each is subtle and very difficult to hold constant. In the living organism, intervening activities are always present; a shift of visual fixation or of posture may change the stimulating context; and these shifts in fixation or posture, or changes in the flow of the subject's symbolic processes, may alter set. The moment-to-moment changes in the organism inevitably mean changes in all three of these conditions. In spite of these difficulties, it has been possible to keep the variations of the "constant" conditions within a sufficiently narrow margin to prevent major distortions in the results of the conditions being varied. Before discussing the fundamental conditions of the forget-

ting process, however, a word should be said concerning the "law of disuse" in explanation of why such a principle is not considered as a valid and proper explanation of forgetting.

Disuse and Deterioration of the Organic Correlate

It has sometimes been said, and often been implied, that disuse is the fundamental condition of forgetting, i.e., explains it. The reasoning is that learning occurs during practice or use, while during retention intervals, when there is no practice of the act in question (disuse of it), a decrement appears; therefore, the differential lies in the disuse. It is not always easy to tell whether those who write of a "law of disuse" mean that disuse produces forgetting or only that, with disuse, forgetting frequently takes place. The latter merely states the fact of the general decrement during periods free from specific practice at the act in question, but, where the former is meant, it is unacceptable.

The view that disuse produces forgetting is contradicted by a number of potent considerations. In the first place, forgetting and disuse are not correlated to the extent assumed by this view. During disuse, retention curves do not always fall; instead, sometimes they rise. The Ward-Hovland phenomenon of reminiscence, where the increment in retention cannot be considered to be a result of an initial recall, is a case in point. True, a decrement sets in later on, but the fact that an increment occurs after an interval of disuse following practice imposes a limitation on disuse as a determining condition. Likewise, conditioned responses recover during a period of no practice following extinction.

A further limitation is placed upon the determining influence of disuse by the fact that forgetting may take place during use or practice. Conditioned responses are extinguished by continued presentation of the conditioned stimulus without reinforcement. Similarly, subjects become negatively adapted so that responses, initially elicited, are called forth no longer. The conditions during extinction and negative adaptation are different from those during practice, but there is repetition or use of the response, and during

this repetition without reinforcement, the response diminishes and disappears. During serial learning, whether verbal or perceptual-motor, correct responses appear, are forgotten, and appear again. The first perfect repetition of a list of words is commonly followed by one or more incorrect repetitions. Habits may be eliminated by repetition under certain conditions. Meaning may lapse with continued fixation of a word, and in the phenomenon of refractory phase, repetition raises a barrier against immediate repetition of the same response.

Even if disuse were not sometimes followed by an increment and use sometimes by a decrement, forgetting could not be accounted for by disuse, if disuse means only the passage of time, for time, in and of itself, is not a determining condition of events in nature. It is a conceptual framework in which activities go their ways and in terms of which events are plotted. In time, iron may rust and men grow old, but the rusting and the aging are understood in terms of the chemical and other events which occur in time, not in terms of time itself.

Over and above these considerations, and experimentally more important, is the fact that *forgetting is found to vary with the character of the events which fill a constant retention interval and with the conditions obtaining at the time of measuring retention.* If, with a constant degree of learning, and after a constant time, amount retained is a function of the conditions mentioned, it is to them and not to time that we must look for an understanding of the forgetting.[25]

There is presumably no one who does not assume that learning modifies the organism, particularly the central nervous system, and that continuation of this modification is necessary for retention. It is a plausible theory, therefore, that forgetting is produced by a deterioration of this organic correlate, substrate, or trace, and that the loss of the behavioral changes which represent learning mirrors this deterioration. Since disuse should be optimal for such deteriora-

[25] A more detailed discussion of disuse and forgetting, together with some of the bibliography in support of the position taken in the text, will be found in McGeoch (1932a).

tion, the theory is often implied by statements about disuse. This view is roughly supported by losses in retention which follow some brain lesions, whether produced in man by accident or in rats and chimpanzees by design. Memory losses in senescence and in other organic disorders where there are known pathological changes in the brain also support this view. The effects of electroconvulsive shock upon retention provide another line of supporting evidence. A number of theories have been developed concerning the specific nature of the deterioration of the organic results of learning.

This type of theory can be stated only in the most general terms. The specific organic correlates of learning are unknown, and a theory of forgetting in terms of their deterioration has little specific information with which to work. Cerebral insult often does influence retention, but this insult is an inruption into the ordinary processes of growth and decay and tells us little about the usual mode of operation of these processes. It tells us primarily that retention depends upon certain aspects of the organism, not that without insult the organic correlate changes to produce forgetting. The known changes following brain lesion are not yet sufficiently specific to bear the weight of anything but a very general interpretation. With continued neurophysiological research, knowledge of the correlates of learning and retention is sure to become more exact, but even so, a theory of forgetting in terms of deterioration of an organic trace will need to account for behavioral facts which seem, at the present time, to be difficult to incorporate into such a theory.

There are, furthermore, several classes of phenomena which weigh against such a theory. One of these is the recall of childhood memories, apparently long unrehearsed. This has been found with subjects of varying ages from adolescence through maturity to old age, and is often especially striking in the very old who revert to memories of childhood. Recall under abnormal conditions, of experiences of many years before which have not been reinstated (or which have been reinstated only infrequently) in the interval, constitutes a second class of evidence. Recall by the aid of free association, under hypnosis, in delirium, excitement, and states of anesthesia, are examples of evidence of this kind. There are, as we

have seen, a number of cases of high retention by normal people of material learned many years before and rehearsed little, if at all, in the interval. Why has not a deterioration of organic traces, if at all general, prevented these recalls?

Also potent against this theory is the fact, already mentioned as evidence against disuse, that forgetting may be made to vary from a relatively low value to a very high one by controlling conditions of interpolation and by varying the circumstances under which retention is measured. This does not prove that there is no deterioration of the organic trace, but it does demonstrate that forgetting is determined by other conditions than this, and, taken together with the other classes of evidence already mentioned, it serves to cast doubt on the deterioration theory.

There is, at present, little specific evidence in support of this theory, and such evidence is difficult to obtain. Since forgetting cannot be controlled or predicted in terms of it, but can be controlled in terms of a behavioral theory, it is a more fruitful procedure to work at the behavioral level. The remainder of this chapter will deal with the psychological data relevant to an explanation of forgetting at the level of the data to be explained. The facts themselves are important as phenomena of learning and retention, but they gain in significance by being seen in the context of a general theory.

Interference by Intervening Activities: The Phenomenon of Retroactive Inhibition

The experimental work on forgetting as a function of intervening activity has nearly all been done under the name of "retroactive inhibition," following the usage of Müller and Pilzecker (1900). The term refers to a *decrement in retention resulting from activity, usually a learning activity, interpolated between an original learning and a later measurement of retention.* The expression is not, however, to be taken literally. It is not retroactive in the sense that interpolated activities work backward in time. The term "retroactive" is an *as if* modifier, meaning that it is as if this occurred. In such a metaphorical sense the term can mislead no more than can

the term "refractory phase" or any other such clearly understood terms in related fields.

The concept of retroactive inhibition is more explicitly defined in terms of the operations by which it is measured. Schematically, the experimental paradigm is as follows:[26]

Control condition	Learn 1	Rest	Measure retention of 1
Experimental condition	Learn 1	Learn 2	Measure retention of 1

The subjects learn a given activity or material to some criterion and, after an interval, are tested by one or more of the methods of measuring retention.[27] This is done in the same way under both conditions, the conditions differing only with respect to the character of the subject's activity during the retention interval. Under the control (or rest) condition, there is no formal practice, but instead, the subject reads jokes or indulges in some other form of non-learning activity. Complete rest, in the sense of a psychological vacuum, would be desirable, but since that is impossible, it is necessary to specify the rest-interval activity in order that rehearsal and other experimentally undesirable effects may be controlled. Under

[26] Irion (1948) has suggested the desirability of adding a second control condition to this design under which the subject would learn and then have an immediate test of retention. This would allow an estimate of the amount of forgetting that occurs during the rest interval of the control condition.

[27] When all subjects go through all of the conditions of an experiment in a counterbalanced order and when retention is measured by relearning, it is a necessary experimental precaution, after the relearning of the control activity, to have a second listed practiced to the same degree of learning as that given the interpolated list under the experimental condition. This would be unnecessary if all subjects were at a practice level with the material used—that is, a level of performance which the practice afforded by the experiment will not alter—but such a practice level is seldom attained either during the initial practice or before the beginning of the practice under the separate conditions of the experiment (cf. Melton and Von Lackum, 1941). It is also an excellent precaution, when there is more than one experimental condition, to give the subjects at least two relearning trials of the interpolated activity in order to prevent giving the impression that the interpolated activity is less important than the original one and hence to be practiced with less active intent to learn. Although most experiments on retroactive inhibition have employed a counterbalanced practice order, the dangers of this method, despite the precautions listed above, are considerable (cf. Chap. I).

the experimental (or work) condition a formally interpolated activity is introduced at some point during the interval between the close of practice on the original material and the later test of retention. The interpolation is usually the learning of something else. The remainder of the retention interval is filled in the same way as is the entire interval under the control condition.

Care is taken to insure equivalent degrees of original learning under all conditions. The lengths of the retention intervals are always held constant. The only variable allowed to operate is the interpolated activity under the experimental conditions. Any significant differences in retention between the experimental and control conditions must, then, be referred to the interpolation. Over a wide range of circumstances and interpolations, the experimental conditions yield the lower retention values which constitute retroactive inhibition.

The early work on retroactive inhibition was done with lists of nonsense syllables and much of the subsequent experimentation has employed verbal materials of some kind. The decrement from interpolation is not confined to disparate serial lists, however, but occurs when the original material is poetry, prose, advertising materials, a stylus maze, a pursuitmeter habit, and other materials.[28]

The phenomenon has been found with human subjects, both children and adults, and with animals. It appears also by all of the methods of measuring retention except transfer, which has not been directly used for this purpose. Heine (1914) reported that testing of retention by the recognition method failed to yield a decrement, but later work has repeatedly found that recognition will show one (J. J. Gibson, 1934; McKinney, 1935; Zangwill, 1938). It may be concluded that the phenomenon called retroactive inhibition is very general. It represents experimentally produced forgetting, and the conditions which determine its amount are among the most important in the field of retention. A satisfactory theory of it is a theory of a major part of forgetting.

[28] A review by Britt (1935) contains a discussion of, and references to, the work in this field through 1934. A more recent review by Swenson (1941) surveys the literature through 1940. Each of these papers contains extensive bibliography.

Conditions of Which Amount of Retroactive Inhibition Is a Function

DEGREE OF SIMILARITY BETWEEN ORIGINAL AND INTERPOLATED ACTIVITIES

The large number of possible relations between the original and interpolated activities in the retroactive inhibition paradigm have been most often studied under the head of similarity. This usage may be followed if it is remembered throughout that similarity is not a single characteristic but a class of conditions which has many dimensions. The two activities may have varying degrees of identity; they may be alike in meaning (and this, in turn, may be classified in various ways); they may occur in similar serial orders; they may be practiced by similar methods; and so on.

Reasoning from the results of experiments of his own (1920) and of Skaggs (1925), in which the original and interpolated materials had not been dimensionally analyzed, Robinson (1927) formulated a generalization which he suspected to hold for a wide range of conditions and which read thus: "As similarity between interpolation and original memorization is reduced from near identity, retention falls away to a minimum and then rises again, but with decreasing similarity it never reaches the level obtaining with maximum similarity." This relationship is illustrated in Figure 45.

Since the relationship is based upon the results obtained by Skaggs and Robinson, we may call it the Skaggs-Robinson hypothesis.[29] One shortcoming of this hypothesized relationship is that it fails to differentiate stimulus and response similarities. A more fruitful type of theorizing would distinguish between similarity of the eliciting stimuli and similarity among the responses to them. Such an hypothesis has been worked out by Osgood (1949) and has been shown in Figure 34 (Chap. IX). According to this view, maximum facilitation occurs when the stimuli and responses are identical in original and interpolated learning. If the stimuli remain identical

[29] Boring (1941) has discussed the rationale of the Skaggs-Robinson function in terms that are applicable to other than the serial materials with which his note is directly concerned.

while varying degrees of response dissimilarity are introduced, facilitation quickly gives way to inhibition, this inhibition becoming maximal as direct antagonism between the responses is approached. On the other hand, with identical responses, a decrease in stimulus similarity causes a falling off of the facilitating effect toward zero. With dissimilar or opposed responses, decreases in stimulus similarity result in a reduction of inhibition toward zero. Let us briefly summarize some of the results which have been obtained when different dimensions of similarity have been systematically varied.

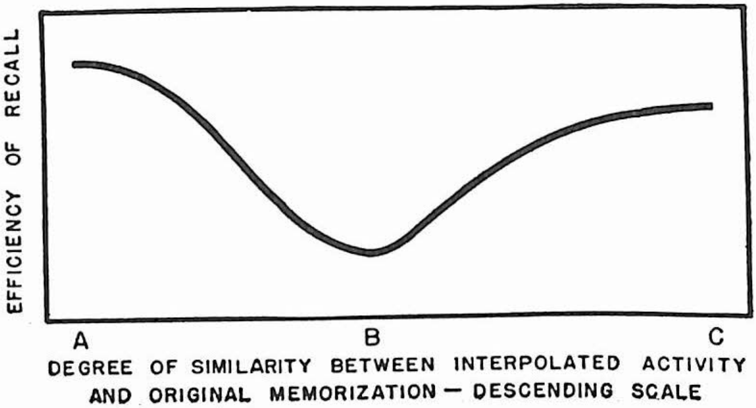

FIG. 45. SKAGGS-ROBINSON HYPOTHESIS CONCERNING RELATION BETWEEN SIMILARITY AND EFFICIENCY OF RECALL

(From Robinson, *Amer. J. Psychol.*, 1927, 39, p. 299)

At A, where similarity between interpolated activity and original memorization is at a maximum (where they are as nearly as possible identical), recall is at its highest level of efficiency. As this similarity approaches its minimum at C it passes through an intermediate degree, B, where recall is exceedingly inefficient. After this point is passed, there is an increased inefficiency of recall, but this increase does not return to the level obtaining at A.

(A) *Formal similarity (identical elements).* Maximal identity of original and interpolated materials is attained when the original list continues to be practiced during the period arbitrarily assigned to the interpolation, this practice then being regarded as practice at an interpolated activity. Under such circumstances both formal and meaningful similarity (and usually the other dimensions, as well) are maximal. This is a condition which yields facilitation

(Irion, 1946), and both the Skaggs-Robinson hypothesis and Osgood's theory legitimately place the region of maximal recall in this region of maximal identity.

The dimension of percentage of identity was employed by Robinson (1927) when he attempted to test the Skaggs-Robinson hypothesis. Employing the memory span method with lists of eight consonants, and regarding the two halves of each list as original and interpolated materials, he varied the similarity between the two in terms of the number of identical consonants, with the factor of position controlled as shown in Table XXIV. The experiment has been repeated by Harden (1929), but with degree of similarity defined in terms of the relative numbers of consonants and digits in the second half of the list, the only identity being, therefore, the proportion of items of the same class in the two halves of the span (Table XXIV).

TABLE XXIV

SCHEMA OF THE ROBINSON AND HARDEN EXPERIMENTS OF RETROACTIVE INHIBITION AS A FUNCTION OF SIMILARITY

(From Britt, *Psychol. Bull.*, 1935, 32, p. 393)

Elements in Common	Robinson		Harden	
	Original	Interpolated	Original	Interpolated
None	a-b-c-d	e-f-g-h	a-b-c-d	1-2-3-4
One	a-b-c-d	a-f-g-h	a-b-c-d	e-2-3-4
Two	a-b-c-d	a-b-g-h	a-b-c-d	e-f-3-4
Three	a-b-c-d	a-b-c-h	a-b-c-d	e-f-g-4
Four	a-b-c-d	a-b-c-d	a-b-c-d	e-f-g-h

The results of the Robinson and Harden experiments are given in Figure 46. The Robinson curve falls from a high point of recall (low retroaction), when all consonants in the second half of the span are identical with those in the first half, to a lower point (higher retroaction), when none is identical, showing, thus, only partial conformity to the hypothesis. It does conform to the AB section of the hypothetical curve, but shows no hint of the BC section. Harden's experiment was performed because she held that the Robinson procedure was appropriate to test only the AB section

and that a more adequate test of the BC region could be made by using varying numbers of digits in the second half. A combination of the two curves does roughly resemble the hypothetical relationship of Figure 45. Harden's four-in-common condition (but with

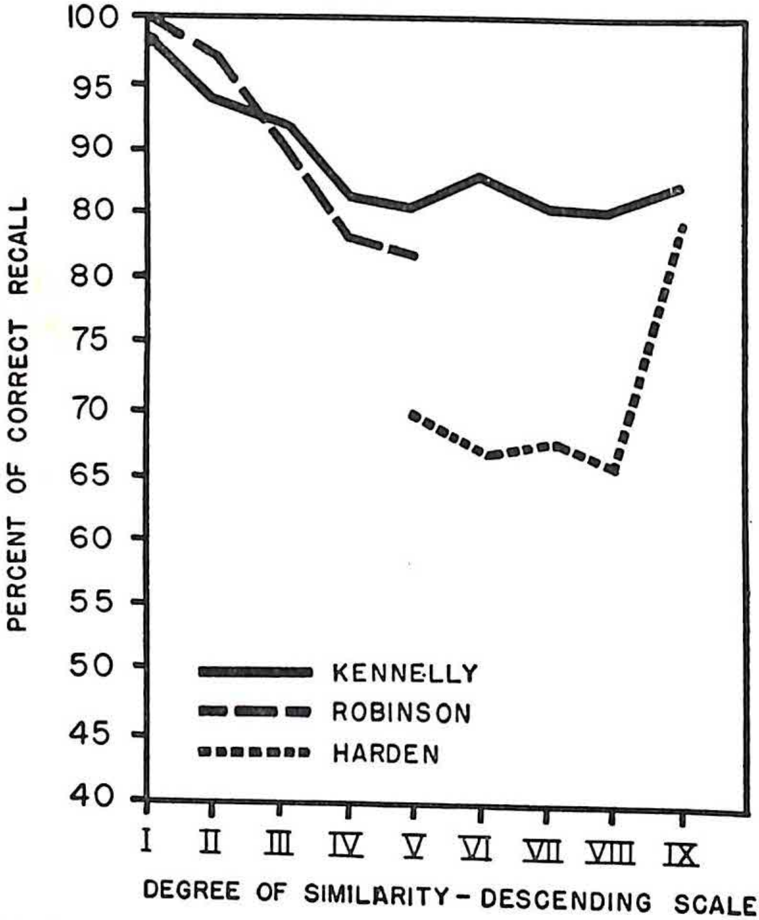

FIG. 46. THREE EMPIRICAL RELATIONSHIPS BETWEEN RECALL AND SIMILARITY OF INTERPOLATED MATERIALS
(From Kennelly, *Arch. Psychol., N. Y.*, 1941, No. 260, p. 19)

none identical) corresponds to Robinson's none-identical condition. The fact that the exact values are different may be referred to differences in the experimental procedures.

More recently, Kennelly (1941) has systematically investigated

the Skaggs-Robinson hypothesis, using the methods employed by Robinson and Harden. He has verified Robinson's results for the AB section of the curve but has failed to verify Harden's results for the BC section. These findings are also represented in Figure 45. An additional experiment, employing a design more like that of the usual retroaction experiment, likewise failed to confirm Harden's findings. The consistent failure to confirm them may be a function of the particular combinations of consonants and numbers employed, the amount of prior training of the subjects, and the difficulty of the interpolated items (Kennelly, 1941). At the present time, therefore, we must regard the results of this line of research as being indeterminate as far as the Skaggs-Robinson hypothesis is concerned.

The results of Robinson and of Kennelly do show, however, that amount of retroactive inhibition increases with decreasing percentages of identity between the halves of a memory span list. Percentage of identity is, however, but one dimension along which materials are distributed. The use of varying percentages of identical units in the two halves of a span list constitutes a dimension intersecting the 100 per cent point of similarity, which is complete identity. The identical units in the second half of the span provide continued practice, and it is to be expected that retention should decrease as identity decreases. One may question whether this range represents a sufficiently wide range of similarity to permit an adequate test of the Skaggs-Robinson hypothesis (cf. Kennelly, 1941). One may also question whether this method is analytic enough to permit isolation of the basic similarity factors which determine retroactive inhibition.

Other experiments have varied the identities in original and interpolated activities and have demonstrated that amount of the resulting decrement is a function of degree and kind of identity. Illustrative experiments are those of Cheng (1929) with identities of spelling of nonsense syllables, of B. Watson (1938) with varying identities of compartment and cue in card-sorting, and of Dreis (1933), in which the original and interpolated activities were code substitution, the interpolated code having 0, 2, 3, or 4 items in

common with the original, and in which the results resemble those obtained by Robinson (1927) and Kennelly (1941).

(B) *Similarity of meaning.* Proportion of identical features can be quantified with relative ease in many activities, but when we turn from identity to similarity, and especially to similarity of meaning, the problem of quantification becomes much more difficult. It has been attacked, however, by methods of ranking and scaling. Learning of separately practiced lists, rather than the memory span method, has been used. In an early experiment on this dimension, two-syllable adjectives were employed as original materials, and synonyms of these adjectives, antonyms of them, adjectives unrelated to them, nonsense syllables, and three place numbers were used as interpolated materials (McGeoch and McDonald, 1931). These materials were ranked by judges in the order given with respect to their similarity to the original lists. The recall scores of the original lists rose steadily from a low point when the interpolated lists were composed of synonyms to a much higher point when the interpolated lists were made up of three place numbers. Trials to relearn to one perfect recitation gave the same regular, direct relation between degree of roughly judged similarity and amount of inhibition.

Adjectives and their synonyms were then classified in three degrees of synonymity by eighty judges. The results show that when all interpolated lists contain synonyms of the original lists, but when the closeness of this relation is subdivided into three sharply separated groups, retention is inversely related to degree of closeness of relation, which is the same as saying that retroactive inhibition varies directly as does similarity. There is no hint of the Skaggs-Robinson relation here or in many other experiments.[30] This may be because the conditions of such experiments are arranged to measure the Skaggs-Robinson relationship over only one portion of it, but an equally plausible explanation is the one advanced by Osgood (1949). This explanation notes that McGeoch and McDonald (and

[30] Analogous results have been reported by L. M. Johnson (1933), who used lists of twenty abstract nouns as original materials and lists of their synonyms, grouped in three degrees of synonymity, as interpolated materials.

others) simultaneously varied both stimulus and response similarity from neutrality of both to high similarity of both. This double variation is an inevitable result of using the method of rote serial anticipation learning. As can be seen from an examination of Osgood's (1949) transfer surface (Fig. 34), such manipulation should result in increasing amounts of negative transfer or retroaction. In another experiment, Osgood (1946) has used paired-associate learning. The stimulus items remained the same during original and interpolated learning, but the response items varied in similarity of meaning. Three retroaction conditions were used. In one case, the response items in original and interpolated learning had similar meanings (elated-high); in a second condition neutrality prevailed (elated-left); and in the third condition, the response items had opposed meanings (elated-low). The method of equated learning (Gillette, 1936; Sand, 1939) was employed. Osgood's results show that, as response similarity increased (the stimuli remaining identical) amount of retroaction decreased. This result, it will be seen, may be predicted from the theoretical surface in Figure 34.

Another dimension of similarity of meaning has been explored by Sisson (1938), who employed lists of nonsense syllables of 00 per cent and 100 per cent association value in the Glaze calibration (cf. Chap. I) and found the interpolation of a list of the same associative value as the original list (whether 00 per cent-00 per cent or 100 per cent-100 per cent) to produce more inhibition than interpolation of a list of different value. The results are complicated by the fact that lists of the two different association values required different numbers of trials to learn to the criterion of six out of ten items, but the general finding may probably be accepted.

(C) *Similarity of task or operation.* The relations between original and interpolated activities may vary with respect to the operation to be performed or the method of learning as well as with respect to similarity of content or material. Similarity of operation or method thus constitutes another dimension in certain regions of which, at least, inhibition increases with increasing similarity.

Gibson and Gibson (1934) have shown that similarity of either material or method yields a greater decrement than similarity of

of decrement and length of list found small but regular decreases in inhibition as lists of syllables increased from 9 to 18 items and as lists of three-place numbers increased from 6 to 10, the interpolated lists being constant at 12 syllables and 6 numbers (Robinson and Heron, 1922; Robinson and Darrow, 1924). It is possible, however, that this result should be ascribed to increasing degrees of learning of many of the items rather than, independently, to the length of list, because difficulty of learning increases disproportionately as length increases and, therefore, the longer lists would have the higher average degrees of learning and be less susceptible to retroaction.

Sand (1939) has tested this possibility with original lists of 6, 8, 10, 12, and 15 pairs of nonsense syllables, an interpolation of 7 pairs, and the Woodworth method of dropping out pairs from the list when they have been correctly recited a specified number of times, in this case, three. This method prevents variable degrees of correct frequency in the recall of each second member of a pair. Under these conditions, the four longest lists gave equivalent amounts of retroaction when retention was measured by recall, and the three longest lists did so when retention was measured by saving scores. This permits the inference that the decreases in inhibition found by Robinson, Heron, and Darrow were, to a large extent, functions of the increasing degrees of learning which accompanied increases in length of list, and that increases in length from a point at or just beyond the memory span (under the conditions used) yield approximately equal amounts of inhibition. This does not account for all of the variables involved, of course, because isolation of the influence of length from that of degree of learning has not necessarily been achieved, but it states the influence of one major variable.

AMOUNTS OF INTERPOLATED MATERIAL

Either the length of an interpolated list or the number of interpolated lists may be varied, and each constitutes a variation in amount of the interpolated material. It also constitutes, in a sense,

a variation in the amount of interpolated learning. Increasing the length of an interpolated list also increases the amount of time spent in interpolated learning if trials are held constant, or reduces the number of trials if time is held constant. It is not, therefore, possible to vary all of these conditions independently.

The fact that amount of inhibition is a function of the relative lengths of original and interpolated lists of adjectives implies that amount of interpolated material is an effective variable. Interpolation of 16 adjectives after an original list of 16 produces a greater decrement in retention than does any interpolation of 8 adjectives, regardless of relative frequency, when the frequencies applied to each length of interpolated list are 4, 8, and 16 (McGeoch, 1936). When both lists contain 16 items, the possibilities of confusion are greater than when the interpolated list is half as long. It seems probable that amount of interpolated material, relative to that of original material, is a determiner of amount of retroaction inhibition.

TABLE XXV

VARIATIONS IN RECALL AND RELEARNING WITH NUMBER OF INTERPOLATED LISTS

(From Twining, *J. exp. Psychol.*, 1940, 26, p. 493)

	Number of Interpolated Lists				
	1	2	3	4	5
Mean recall score	1.40	1.13	.41	.27	.07
Mean trials to relearn	5.53	6.93	9.74	11.33	13.53

The effectiveness of amount of interpolated material is further established by the work of Twining (1940), who has defined amount in terms of number of lists of 8 nonsense syllables. An original list learned to 1 perfect trial was relearned after 30 minutes during which either 1, 2, 3, 4, or 5 lists had been given 10 presentations each. These interpolated presentations came in the middle of the total retention interval, the remainder of which, before and after the interpolated practice, was spent in a game which constituted the "rest" activity. The recall and relearning records (Table XXV) exhibit decrements which vary directly with the number of interpolated lists. The decrement in saving score produced by five

interpolated lists is approximately the same as that found by Ebbinghaus after 24 hours without formally interpolated activity. Twining's results are closely supported by the findings of Underwood (1945). In Underwood's experiment, retroactive inhibition was studied when 2, 4, or 6 lists (each given 4 repetitions) were interpolated between original learning and relearning. In a companion experiment, retroactive inhibition was studied under circumstances where a single interpolated list was repeated either 8, 16, or 24 times. Thus, Underwood is enabled to make a comparison of amount of interpolated learning considered as number of lists with amount of interpolated learning considered as repetitions of a single list, this comparison involving a constant number of trials. It is apparent that the results of this experiment are closely related to the findings regarding retroactive inhibition as a function of frequency of repetition of the interpolated activity. Over the range of conditions employed, Underwood found a greater inhibitory effect from increasing the number of interpolated lists than from increasing the number of repetitions of a single list by an equal amount. His results also corroborate those of Twining (1940) in that they show retroactive inhibition to increase directly with the number of interpolated lists learned.

SLEEP, INACTIVITY, AND OTHER METHODS OF FILLING THE RETENTION INTERVAL

The conventional experiment on retroactive inhibition compares retention after an interval filled by some non-learning activity with retention after an interval at least partly filled by some specific learning activity. The forgetting produced by the interpolated learning is much greater than that which accompanies intervening activity of a non-learning sort, but the question remains whether the activities of daily life also have inhibitory potency. It is to be noted that under the rest conditions of many retroactive inhibition experiments, considerable losses of retention may occur (cf. Irion, 1948). Akin to this is the question of whether the decrement obtained under the control condition is a function of the informal ac-

tivities which fill it. The answer is a crucial point in a theory which ascribes major portions of forgetting to interference from intervening activities.

The first major experiment on this problem was performed by Jenkins and Dallenbach (1924), who compared the recall of lists of 10 nonsense syllables after 1, 2, 4, and 8 hours of ordinary waking activity with recall after similar periods spent almost entirely in sleep. This experiment not only tells us whether the activities of daily life will inhibit the recall of materials presumably unrelated to them, but also to what extent forgetting occurs when the retention interval is as free from activity as it can be made without artificial means such as anesthesia. The two subjects and the experimenter slept in the laboratory. If retention was to be measured after an interval of sleeping, the practice began when the subject was ready for bed. Upon reaching the criterion of one perfect trial, the subject retired to be awakened by the experimenter for a test of recall after the desired interval.[31] Only one interval was used per night. The learning for retention after intervals of waking was done during the day, the subjects returning to the laboratory for recall after the appropriate interval. For both subjects the curve of recall after sleep drops during the first two hours, though not as rapidly as the curve after waking, and from that point to eight hours shows no further decrement, while the corresponding curve after waking continues to fall until after eight hours the two are widely separated (Fig. 48).

Van Ormer (1932) has repeated most of the conditions of the Jenkins and Dallenbach experiment but has employed the saving method as a measure of retention. His results lead to the same general conclusion. He has also reviewed a number of other and less systematic experiments, which corroborate the implications of

[31] It might seem that being awakened at an unaccustomed time of the night, brought to another room, and asked to recall would be an unfavorable condition. The subjects often came to the experimental room, recalled well, went back to bed, and failed to remember in the morning that they had been awakened. In spite of great drowsiness at the time of recall, recall was relatively high, so that any decrement from that condition must have been comparatively small. These results have significance for any theory of the nature of sleep, and particularly for those theories that regard sleep as a cumulation of inhibition.

those already mentioned (1933). There seems to be no doubt that nonsense syllables are better retained after intervals of sleeping than after corresponding intervals of waking. From two hours to eight hours the retention curves under the sleep condition show no

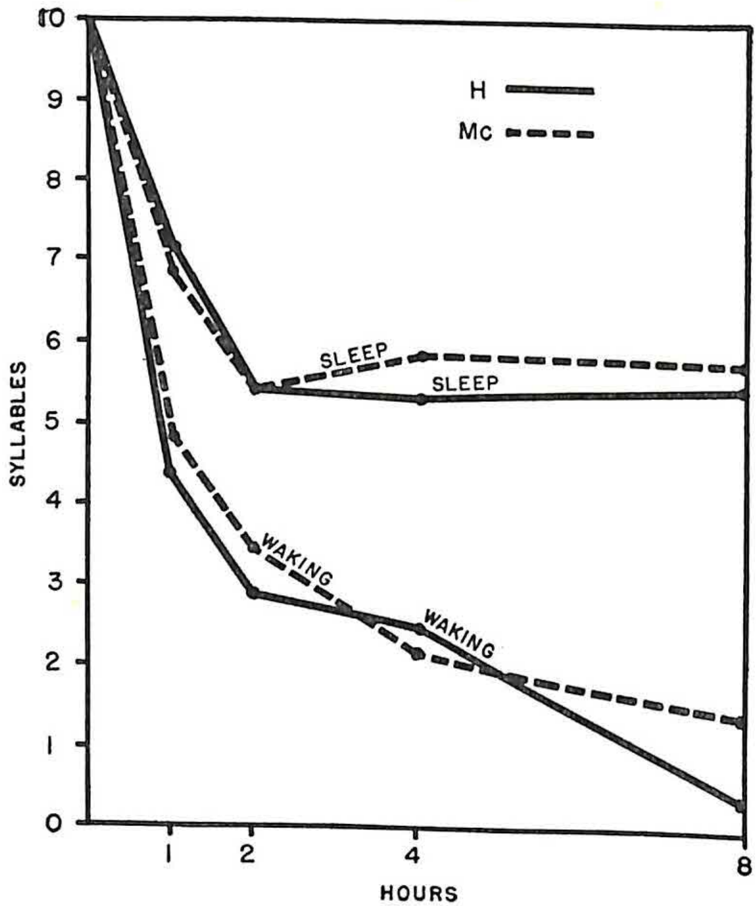

FIG. 48. AVERAGE NUMBER OF SYLLABLES RECALLED AFTER INTERVALS OF SLEEPING AND WAKING

(From data of Jenkins and Dallenbach, *Amer. J. Psychol.*, 1924, 35, p. 609)

significant decrement, while those under the waking condition have the customary continued fall. During the first two hours, however, the curves under the sleep condition also fall, although not quite so rapidly as those under waking.

The most reasonable interpretation of these results is that ordinary waking activities serve to inhibit the retention of the originally learned lists. During sleep these everyday activities are absent and, therefore, retention is higher than it is after waking. The process by means of which this inhibition occurs is not entirely clear, however. Many of the activities of everyday life are quite dissimilar to the laboratory activities employed by Jenkins, Dallenbach, and Van Ormer and, on this basis, little retroactive inhibition would be expected. The factor of set to perform, which is discussed in a later paragraph, may well be of importance in explanation of these results. It has been customary to regard the data of Jenkins, Dallenbach, and Van Ormer as continuous with those obtained in the conventional experiments on retroactive inhibition, the sleep condition being the control and the waking condition the experimental one.

The initial drop of the retention curves under the sleep condition is a problem for this interpretation. Are there conditions operating, over and above the effects of retroactive inhibition, to bring about this decline? Physiological changes, now only remotely guessed, might do it. Loss of muscular tension during sleep might result in a lowered performance due to a lessened dynamogenic effect (cf. Chap. VI). Part of the initial decline may be a function of the activities of leaving the experimental room, going to bed, and getting to sleep. To fall asleep probably required only a few minutes, yet the retention curve falls to the two-hour point. The return to the experimental situation after an interval of one or two hours might fail to arouse adequately the original set of the subject and the full response to the original context, with a resultant failure of retention. This might be roughly a constant after each interval, but be abetted by interference from intervening activity in the early minutes before going to sleep and throughout the waking intervals. This possibility is supported by the fact that the forgetting after one and two hours in the Jenkins and Dallenbach sleep condition was less than in the waking condition.

What is needed is a psychological vacuum, or period of complete absence of activity, stretching from the moment of reaching the

criterion of original learning to the moment of beginning the measurement of retention. If no decrement appears, a strictly psychological interpretation, of whatever kind, will be required. Attempts to attain this state of vacuum with animals have not been uniformly successful. Hunter (1922) attempted to control the activity of cockroaches by means of temperature (cockroaches become immobilized by cold). His results indicated that inactivity produced less retention than activity (normal temperature), but the effects of cold may be detrimental to the functioning of the animal and the process of thawing has an indeterminant effect. Drugs have been used with varying degrees of success. Russell and Hunter (1937) found no difference in the retention of rats anesthetized by sodium amytal and unanesthetized rats. Other attempts to control activity level by means of temperature have yielded results which are not entirely clear-cut (cf. Hoagland, 1931; French, 1942). On the other hand, Minami and Dallenbach (1946), using the fact that cockroaches tend to become immobilized when subjected to extensive bodily contact, demonstrated that inactivity has a markedly beneficial effect upon retention of a simple maze habit in that species. These results are the most comprehensive ones concerning the effects of activity upon retention.

The conditions of the greater retention over sleeping than over waking periods have not been adequately worked out, though one of them, relative meaningfulness, may be mentioned. When subjects reproduce stories after eight hours of either sleeping or waking, the ideas essential to the plot of the story are as well recalled after waking as after sleeping, while the nonessential material is better recalled after sleeping (Newman, 1939). No measures are given of the relative amounts of the essential and nonessential ideas which were actually learned, and in the absence of this information the significance of the results is difficult to assess. The slight susceptibility of the more meaningful (essential) parts of the stories to forgetting after either sleeping or waking is to be expected, however, from the experiments on the retention of meaningful materials and from the fact that the ideas in a prose passage and the meaning of lines of poetry are relatively little susceptible to interference from

other materials of a similar kind (McGeoch and McKinney, 1934a, b).

INSTRUCTION, SET, AND ISOLATION OF MATERIALS

The set of the subject, whether aroused by instructions from the experimenter or in some other way, may be a potent determiner of retroactive inhibition. Whitely (1927) has demonstrated that an apperceptive set congruous with the original material has a greater inhibitory effect than one not congruous with it. These findings are quite similar to the findings of Postman and Postman (1948) on the learning of "compatible" and "incompatible" pairs of words, which have been reported in an earlier paragraph.

Lester (1932) has been able to demonstrate a graded series of results by means of the following instructions, designed to establish a series of sets: (a) expectation of recall; (b) expectation of interpolated material before recall; (c) information about the possible effect of the interpolation; and (d) assumption of an active attitude and an effort to avoid the detrimental influence of the interpolation. The decrements in retention were large when the subjects expected neither recall nor interpolated activity and were given both. They decreased with the special instructions in the order listed, until, with an active attitude to resist the inhibitory effects of the interpolation, they had become much less than under the conditions which carried no special instructions.

Jenkins and Postman (1949) have obtained interesting results when they manipulated the set of the subject by changing the method of learning. When original and interpolated learning both take place by the anticipation method, more retroactive inhibition results than when original learning is by the anticipation method and interpolated learning is by the method of recognition. Similarly, more interference is obtained when original and interpolated learning are both by the recognition method than when original learning is by the recognition method and interpolated learning is by the anticipation method. In all cases, of course, retention is measured by the method used in original learning.

The fact that an effort to resist interference from interpolated

learning can reduce the amount of the interference is of the first importance. It implies a functional isolation of the original and interpolated materials in a manner to decrease the interrelations between them. It resembles functionally, and may be on a continuum with, the isolation achieved by relatively low degrees of similarity, different methods of learning, different degrees of learning of the two lists, or very high degrees of learning of both of them. It may also be on a continuum with three results obtained by Nagge (1935): (a) that less interference appears when the original list is learned in an hypnotic trance and the interpolated list is learned in the waking state, or the reverse, than when both are learned in the same state (both hypnotic or both waking); (b) that inhibition is less when the original list is learned and relearned through one sensory modality and the interpolated list through another than when both lists are presented to the same modality; and (c) that the decrement is less when the subject, who wrote the material as it was learned, wrote the two lists with different hands. These results, and others, imply that retroactive inhibition decreases with an isolation of the interpolated activity from the original, an isolation which may be brought about in a number of ways, among which instruction and set are very influential.

THE METHOD OF MEASURING RETENTION

The two methods of measuring retention which are most often used in studies of retroactive inhibition are recall and relearning, and, of these, recall is more sensitive in the sense that it usually yields a greater decrement. This is because of the beneficent fact that retroactive inhibition is usually quite *transitory*, the effects of it tending to dissipate during the first few relearning trials. The first relearning trial (recall 1) may show a very substantial difference between the experimental and control conditions, the second relearning trial (often called recall 2) regularly shows a smaller difference, until by the third or fourth trial the control and experimental conditions may be equivalent. Melton and others have made careful analyses of this transitoriness. In general, its amount

has been shown to be a function both of the frequency of repetition of the interpolated lists and of the similarity between the original and interpolated lists. Retroactive inhibition persists throughout relearning of the original list of syllables when the interpolated list has been given a frequency of 10, but disappears after 4 or 5 trials when a frequency of 40 has been used (Melton and Irwin, 1940). These results are only partially substantiated by the results of Thune and Underwood (1943) who only carried interpolated learning to 20 trials. Their results indicate that retroactive inhibition is extremely transitory for all amounts of interpolated learning used (2, 5, 10, and 20 trials). Results showing more rapid dissipation of retroactive inhibition with high degrees of interpolated learning have been obtained by Underwood (1945).

The transitoriness of the decrement from intervening activities permits us to understand some of the instances of forgetting in everyday life and in educational practice. For example, the common experience that, although recall of a particular skill may be at or near the zero point, a very small amount of practice is necessary to bring the habit back to a high level of performance, is an example of this phenomenon.

OTHER VARIABLES

(A) *Temporal point of interpolation and length of retention interval.* Interpolated activity may be introduced at any point during the retention interval, the latter may be of any length, and the two may be expected to be interrelated. Different experiments with different conditions have found each of the three points of interpolation (just after learning, just before recall, and intermediate) to yield the greater amount of retroaction (cf. Britt, 1935; Swenson, 1941). The relationships between point of interpolation, length of interval, and the other variables of this type of experimentation have not been sufficiently worked out to permit any generalized statement concerning point of interpolation. Enough is known, however, to assure us that the two variables under discussion are worth further study.

(B) *The age of the associations.* Some of the results on the preceding problem have suggested that the age of the associations involved is a significant variable, and Britt and Bunch (1934) have demonstrated that it is. The younger of two maze habits of equal strength, as measured by the criterion of mastery, is the more susceptible to interference from an interpolated maze. The variable of age is inevitably present in the preceding problem as well, when interpolations at different points in an interval as long as twenty-four hours are compared.

(C) *Electric shock during either original or interpolated learning.* Administration of shock for errors in the original learning of a stylus maze markedly reduces retroactive inhibition, and shock during interpolated learning, original learning having been without shock, likewise reduces inhibition (Bunch and McTeer, 1932). This effect may be produced by the increase in degree of learning and performance brought about by the shock motivation and by the reduction in the degree of similarity and increase in isolation of the two mazes when one is learned with shock and the other without it.

(D) *The CA and IQ of the subjects.* Lahey (1937) has found an irregular tendency for amount of inhibition to decrease with increasing CA from 8 to 16. When the subjects are grouped in intervals of two years of CA and in three IQ intervals (above 105, 95-105, and below 95), degree of inhibition appears to be inversely related to IQ.

(E) *Other conditions of interpolation.* From what is known of learning and retention it is to be expected that the appearance of a decrement in retention and its amount will be a function of the intervening events. The introduction of reading aloud, reading with a noise and a flash of light, or reading with shock or threatened shock, loss of bodily support and noise, all yield a decrement (Harden, 1930). Some inhibitory effect is exercised by unpleasant odors, while pleasant odors have a facilitating effect (Frank and Ludvigh, 1931). The differences involved in these, and many other, experiments on interpolated conditions not involving learning have been small.

PSYCHOPHYSICAL JUDGMENTS AND RETROACTIVE INHIBITION

Since the early days of psychophysics there have been hypotheses which implicitly connected psychophysical judgments and memorial functions. Whenever a second stimulus is to be compared with a first, the retained effect of the first must be carried over by the subject to be used in making the judgment when the second is presented. It remained for Pratt (1933, 1936) to formulate explicitly the continuity between psychophysical judgments and retroactive inhibition data, which he has done in the setting of the negative time error.

When subjects are asked to compare two objectively equal stimuli with respect to intensity, and when the two are presented successively, the second is judged to be more intense than the first in a greater than chance proportion of the judgments. This overestimation of the second stimulus is known as the negative time-error. Fechner ascribed the phenomenon to a diminishing image of the first stimulus. Such an assumption clearly places the negative time-error in the category of memorial function. If another stimulus is interpolated between the two comparison stimuli, the experiment takes the form of the retroaction paradigm with very simple materials and short intervals. Pratt believes that the psychophysical operations permit a more decisive test of theories of forgetting than do those of the conventional retroaction experiment, which works with relatively complex materials. However this may be, the continuity of the two often-separated fields is apparent (cf. Irwin and Seidenfeld, 1937; Postman and Page, 1947).

A few of the relevant experiments should be described. Lauenstein (1933), for example, interpolated between the standard and comparison stimulus either a more intense or a less intense stimulus than the standard but of the same modality. When the interpolated stimulus was more intense than the standard, a positive time-error appeared, but when it was less intense, the time-error was negative. Similarly, Pratt has interpolated a less intense auditory stimulus, using a similar auditory stimulus as standard and comparison, and has obtained a clear negative time-error. Another ex-

periment, in which the stimuli were weights and the interpolated stimulus a lighter weight, gave a negative error. Pratt also cites data from Guilford and Park (1931), who used a standard stimulus of 200 grams and interpolated weights of 100 grams or of 400 grams, and found large negative time-errors with the lighter interpolation, but a slight effiect in the opposite direction with the heavier interpolation.

Pratt uses these data to defend the theory that forgetting is a function of two conditions: (a) interaction between traces, which is essentially retroactive inhibition, since he means interaction between the organic correlates of original and interpolated stimuli, and (b) distintegration or weakening from endogenous causes. The first assumes that, if the interpolated stimulus is less intense than the standard, the traces of the two interact and, as a result, the trace of the standard is brought down to a lower level than it would have reached without interpolation. The comparison stimulus finds it at this lower level and is therefore judged to be more intense. The concept "trace" as Pratt employs it, carries no reference to specific physiological locus or process; it is a construct from the empirical data. The second process (weakening from endogenous causes) assumes a weakening of the traces which occurs as a function of conditions within the trace system.

Pratt contends, on two grounds, that interaction of traces is not sufficient to account for the data. (a) When the interval between the two stimuli is formally unfilled, as in the control condition of the retroaction experiment, the negative time-error is smaller than when a weak stimulus intervenes. If interaction, or assimilation, were the only effective condition, the error should be greater when there is no interpolation; that is, assimilation to zero should exceed assimilation to an intensity greater than zero, but it does not. (b) Assimilation to a stimulus more intense than the standard is less than assimilation to one less intense than the standard. If assimilation were the only determiner, the positive time-errors should equal the negative time-errors.

To account for these facts, Pratt hypothesizes a weakening or disintegration of traces analogous to a principle of disuse. This

disintegration, together with interaction, determines forgetting. Disintegration is supposed always to be operating; interaction occurs only when there is another stimulus in the comparison situation sufficiently similar to induce interaction. The greater negative time-error with a relatively weak interpolated stimulus than with none is explained by supposing that, without interpolation, disintegration alone is operating. With interpolation, however, both disintegration and interaction are operating. The failure of the positive and negative time-errors to balance is explained in a similar fashion. When the interpolated stimulus is below the standard intensity, disintegration and interaction operate in the same direction; when it is above the standard, disintegration pulls the trace downward, but interaction pulls it upward. In this case, interaction is able to keep the trace somewhere near its initial level but not to compensate entirely for disintegration.

The hypothesis of disintegration from endogenous causes, formulated to account for the preponderance of psychophysical judgments in the direction of increased intensity is a possible one. It is, perhaps, more plausible here, where the time intervals are very short and the original material (the first stimulus) learned to a relatively slight degree, than where the interval is much longer and the material is better learned. Psychological conditions other than those involved in what Pratt calls interaction should also be considered. The set of the subject may be altered, both by an interpolated stimulus and by a brief interval empty of special stimuli. The response to the standard may be expected to be in refractory phase (less likely to be repeated) for a very brief period, thereby giving opportunity for competing stimuli from the environment to elicit other responses which will change the set and perhaps decrease the probability of reinstating the first response. An effort on the part of the subject to hold or continue the first response, in spite of refractory phase and competing stimuli, may also be a condition of the judgment when the comparison stimulus is presented. At the present time, however, both disintegration and any hypothesis in terms of psychological conditions are possibilities. It should be said, nevertheless, that there are other ways of accounting for negative time-errors,

and there are psychophysical results which are difficult to interpret in the way suggested by Pratt. The present discussion has been confined to his view, however, since it is aimed directly at relating psychophysical judgments and the conventional data on retention.

Theories of Retroactive Inhibition

Retroactive inhibition is a case of forgetting which is experimentally produced by interpolating a second learning task between the time of original learning and later recall. This interpolated learning is the experimentally manipulated variable. With changes in it, decrements in retention are found to vary, and, in view of the fact that some learning activity is probably going on during all of one's waking moments, we may infer that retroactive inhibition is a general condition of forgetting. The next step is to inquire how the interpolated learning produces a decrement in the retention of the original material. Theories of retroactive inhibition have sought to formulate the way in which this takes place, or to state the fundamental conditions of this phenomenon. There is, it will be recognized, a great deal of continuity between the conditions already discussed and the theories which use this information in the ways described here.

THE PERSEVERATION THEORY

The first theory to be advanced was that of Müller and Pilzecker (1900) as an outcome of the experiments which brought retroactive inhibition to general recognition. They assumed that the neural or other organic correlate of learning continues or perseverates for a time after practice ceases. This perseveration of the organic events fixates the activity more firmly than it would be fixated without the perseveration. Under the control or rest condition of the retroaction experiment, this perseveration is supposed to continue relatively unhampered by other stimulation which might inhibit or diminish it in some way, and, as a result of this continuation, the activity practiced has the advantage of what amounts to a gratuitous and unintended additional practice. By virtue of this perseveration, the

material is better learned and better retained. The interpolated learning, on the other hand, interferes with the perseveration, and, as a result, the original material is less well retained. If interpolated learning is introduced immediately before recall and relearning instead of immediately after original learning, the perseveration from interpolation may interfere with the recall and relearning of the original. According to this theory, then, optimal retention should occur when interpolated learning is introduced in the middle of a rather long retention interval.

It will be recalled that this theory has also been offered as a possible explanation for the effectivenes of distributed practice and for reminiscence. Its potential applicability to retroactive inhibition emphasizes in another way the continuity of distribution, reminiscence, and retention.

Attempts have been made to specify the character of the perseveration, though without the benefit of direct neurophysiological measurement. It has been envisaged as a continuing afterdischarge of the neurones; it has been thought of as a biochemical lag or other inertia phenomenon; it has been conceived as a reverberating neural circuit; and it would not be inconceivable to locate the perseveration in the muscles. There are, thus, several possible specific perseveration theories, all of which reduce to the general class described above.

On the face of it, some form of perseveration theory is plausible. There are a number of psychological phenomena which imply a perseveration. One calls to mind such phenomena as afterimages, immediate memory images, the running of tunes in one's head, the uselessly repetitive behavior of most children and some adults, and the phenomenon of retrograde amnesia. A theory which accounts for diminished retention in terms of a damping of perseveration would be able to account for the phenomena of psychophysical experiments which Pratt refers to disintegration and could incorporate many of the results of retroaction experiments. It deserves, therefore, careful consideration, the more so because it has also been offered to explain the phenomena of reminiscence and the effectiveness of distributed practice. Nevertheless, the conclusion regarding

it must be that, while it cannot be completely ruled out at the present time, it is, at best, a very partial account of the decrements from intervening learning. A transfer theory is able to incorporate more of the data and is, therefore, more acceptable and more general. But first, a more detailed evaluation of the perseveration hypothesis should be made.

Since there is no direct evidence for a perseveration of the neural units underlying learning, evaluation of the theory must look to the behavioral data. No one has assumed that perseveration continues in any significant amount for more than a few minutes. It follows that interpolation within a very short time after the close of original learning or very close to recall and relearning should yield a decrement, and that amount of decrement should vary with these temporal variables. Interpolation after hours or days, when perseveration must surely have ceased, should produce no decrement. These predictions from the theory have not been experimentally verified (except for the findings of Duncan that are described below). Interpolation at various points during long retention intervals has been shown to produce a decrement, even interpolation midway in an interval of six weeks (Bunch and McTeer, 1932), and there is no evidence for the general positive relation between temporal nearness of the interpolation to the original or relearning and amount of decrement which the theory requires.

Müller and Pilzecker reasonably assumed that the amount of decrement should vary directly with the intensity of the interpolated activity, on the ground that the more intense the interpolation, the greater would be the damping of the perseveration. This is difficult to test directly since we have no direct measure of the intensity of most intervening activities. In a very rough sense, however, it would seem that tapping, electric shock, and color-naming should be in the same region of intensity as practice on another list of words or learning another maze and that, therefore, they should yield approximately as much retroaction as such learning activities. These interpolations give very little decrement, however, and may yield increments (McGeoch, 1931; Irion and Wham, 1951). There is little in the extensive literature on retroaction which can be inter-

preted plausibly in support of the assumed relation between the intensity of interpolation and the amount of decrement.

The fact that amount of retroactive inhibition is a function of the degree of similarity, measured on several dimensions, between original and interpolated activities argues against a perseveration theory. As it is usually stated, the theory makes no assumptions which are relevant to the relations between the two activities and is unable to predict or to incorporate these facts. On the other hand, the facts about retroactive inhibition as a function of similarity strongly support a transfer theory of this phenomenon.

The perseveration theory, as usually stated, offers no explanation for proactive inhibition of retention, where the last learned of two activities is less well retained (Whitely and Blankenship, 1936; Blankenship and Whitely, 1941; Melton and Von Lackum, 1941; McGeoch and Underwood, 1943; Underwood, 1945, 1948, 1949a). To account for this by perseveration, one would need to assume that the perseverating neural substrate of the first activity interfered with the learning of the second activity in ways not apparent from the objective record of performance. This assumption seems unjustifiable and has not been made.

A perseveration theory of retroactive inhibition fails of support on the grounds which have traditionally been urged in its favor and must be regarded as a theory which, if valid at all, can account for only a portion of the facts, but which at the present time cannot be definitely discarded. There are some data which the perseveration theory can explain and which a transfer theory cannot, such as the data interpreted by Pratt in favor of disintegration. Perhaps the most convincing evidence which can be interpreted in favor of the perseveration theory is contained in the papers of Duncan (1945, 1948, 1949) on the effects of electroconvulsive shock upon learning.[32] For example, in his (1949) experiment Duncan trained rats to

[32] There is a growing literature on the effects of electroconvulsive shock on learning and retention. While most of these studies are not immediately relevant to the present discussion, the reader is referred to Finger's (1947) review of the literature and to such later papers as Horowitz and Stone (1947), Braun, Russell, and Patton (1949), Russell (1949), and Townsend, Russell, and Patton (1949). A study, analogous to the study of Duncan (1949), but using human mental patients as subjects, has been reported by Zubin (1941).

perform a simple avoidance habit. During training, electro-shock was administered following each trial, but at varying intervals from the performance of the correct response. The animals were given convulsive shock either 20 seconds, 40 seconds, 1 minute, 4 minutes, 15 minutes, 1 hour, 4 hours, or 14 hours following each learning trial. A control group learned without electro-convulsive shock. A

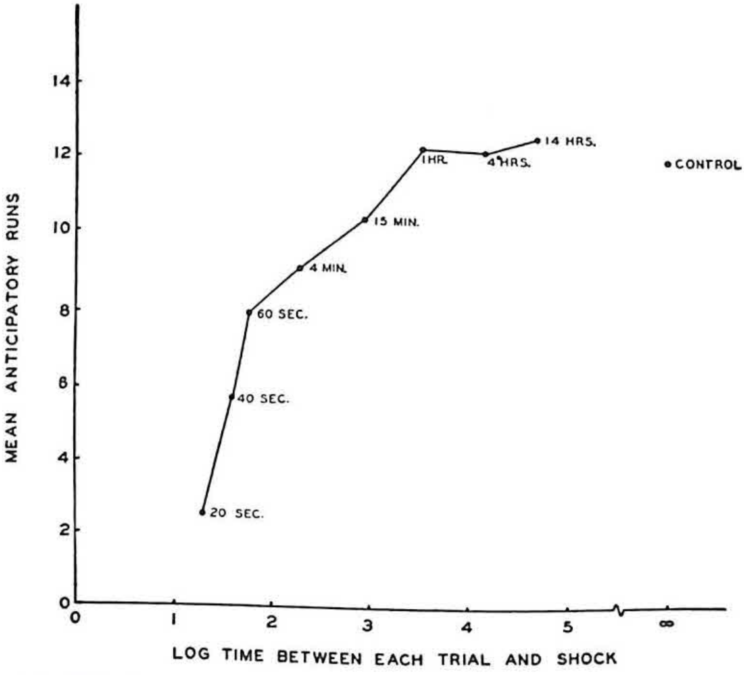

FIG. 49. THE EFFECTS OF ELECTRO-CONVULSIVE SHOCK UPON LEARNING
(From Duncan, *J. physiol. comp. Psychol.*, 1949, 42, p. 35)

Mean anticipatory runs for all trials expressed as a function of the log time interval between each trial and the administration of convulsive shock. No shock was administered to the control group.

separate control experiment indicated that the results could not be attributed to learned avoidance of the convulsive shock. Duncan's findings conclusively demonstrate that electro-convulsive shock, administered after performance, slows down the learning process, and that the amount of decrement is related to the time between performance and the administration of the shock. These results are illustrated in Figure 49. At the present time, it would be difficult

to account for these findings on any basis other than the perseveration theory. On the other hand, little is known about the action of electro-convulsive shock and it may turn out that Duncan's findings can be interpreted in some other way. At the present time, however, the perseveration theory must be retained, even though it is incapable of handling many of the data of retroactive inhibition.

A TRANSFER OR COMPETITION-OF-RESPONSE THEORY

In its most general form, a transfer theory holds that the decrement in retention produced by intervening activity is a form of transfer from activity to activity or of interaction between the two. It is based upon the experimental facts which show that amount of retroactive inhibition is a function of the similarities between original and interpolated materials. The reasoning is that, if the decrement in retention can be made to vary by varying one or more of the dimensions of similarity, then the decrement must be the result of the interaction of the materials.[33] Since this interaction is inevitably between the retained effects of prior learning of one material and the learning or retention of another, it is to be classified under the general heading of transfer of training. Statements of a transfer theory have varied and have undergone modifications as new data have been reported.[34] At the present time, there are different views concerning the most satisfactory formulation of the way in which transfer takes place, but there is considerable agreement on the general outlines of a transfer theory.

According to the current form of the theory, the decrement in retroactive inhibition is a case of transfer which may either be an overt carrying over of specific responses, called *intrusions,* from one material to the other, or a *transfer effect* without overt intrusions.

[33] It may be well to emphasize that such expressions as "interaction between materials" and "relations between materials" are used for convenience only. The relations and interactions must take place, of course, among the processes in the organism corresponding to the experimentally manipulated materials, but, inasmuch as the latter are the variables with which we deal directly, it is convenient to speak in terms of them.

[34] Earlier statements are given, with references to their sources, in Britt (1935, 1936).

The theory regards the data of retroaction and the data of transfer as continuous. Retroactive inhibition is held to be a form of negative transfer of training.

The extensive evidence that amount of the decrement in retention (in retroaction experiments) is a function of the similarities between the two materials offers powerful, though indirect, support to the transfer theory. Direct support for the theory is provided by the occurrence of relatively large numbers of overt intrusions—that is, responses which would be correct if made at the proper place in one list but which are actually made in the recall and relearning of the other list and are, therefore, wrong responses. These transferred responses take the place of the correct ones and thus decrease the retention score (McKinney and McGeoch, 1935). In one experiment in which complete presentation and written recall were used, the overt intrusions were sufficient to account for at least 25 per cent of the decrement in the recall score. In addition to the intrusions accepted by the subject as correct, there were others, recorded and then scratched out, which mean transfer followed by recognition that the transferred items were out of place. Subjects also report the recall of items from the other list which are recognized as not belonging in the list being recalled and not written down at all. These facts point to a more extensive occurrence of intrusions than is shown by the records of the overt intrusions alone.

It is not to be expected that all of the obtained retroactive inhibition can be accounted for by such transfers of items. Transfer effects, both positive and negative, can occur without actual carrying over of responses, and a portion of the negative transfer in retroactive inhibition might be expected to take place without overt or reportable intrusion of responses. In one of Melton's experiments, for example, the percentages of retroactive inhibition attributable to the intrusion of complete interpolated consonant syllables varied between 27.1 per cent and 1.8 per cent decreasing with increasing degrees of interpolated learning. There were also some intrusions of partial syllables and some intruding syllables which the subjects started to call out and instantly corrected, but intrusions were far from accounting for all of the decrement in retention.

The hypothesis has been advanced by McGeoch and his colleagues (cf. McGeoch, 1936; McGeoch, McKinney, and Peters, 1937) that retroactive inhibition reduces to *reproductive inhibition*, which means the decrement in retention following the connection of a common item with two different responses in succession. Designating the items by letters, let the association A-B be formed, and then A-K. The association A-K will be more difficult to form after the learning of A-B than without this prior learning. This phenomenon is called *associative inhibition*. The retention of A-B will be less after the second (interpolated) learning of A-K, and this decreased retention is called *reproductive inhibition*. Since this form of analysis may be most readily schematized in terms of paired units, the experimental paradigm for retroactive inhibition becomes:

Control condition	Learn A-B	Rest	Measure retention of A-B
Experimental condition	Learn A-B	Learn A-K	Measure retention of A-B

It is customary to speak of the A terms as stimulus terms and of the B and K terms as response terms. Learn A-B could as well be written, learn S_1-R_1 and learn A-K as S_1-R_2. The question may then be raised whether the stimulus and response terms are isolated and atomistic. No assumption is made about their magnitude. They may be as small as a single stimulation of a receptor and the contraction of a single muscle fiber or as large as a total situation and a response which involves most of the organism's musculature. Since experimentation and exposition are impossible without analysis, and since, in the early stages of research on a problem, work with simpler cases is advisable, the experimental work has proceeded with simple materials such as pairs of words and we shall speak in these terms.

When we assume transfer to be a basic concept of retroactive inhibition, we may apply our knowledge of transfer to the understanding of retroaction phenomena. On the other hand, we must accept information concerning transfer completely—that is, all of the relevant facts of transfer must enter our explanation of retroaction. From this point of view, it is apparent that the reproductive

inhibition paradigm as it has been stated is somewhat too narrow, relying as it does upon stimulus and response identities. Such identities must, in a strict sense, be fictional since we may be sure that no stimulus and no response is ever repeated exactly. On the other hand, if we allow similarity to play the same role which it plays in transfer, retroactive inhibition should be predictable from our knowledge of transfer; for example, from the relationships summarized in Figure 34 (Chap. IX). The bulk of the work on retroactive inhibition lends itself to analysis in terms of such a revised paradigm of reproductive inhibition. A few experiments have specifically employed the paradigm with its stimulus and response identities. A few examples from the experimental literature will suffice. McGeoch, McKinney, and Peters (1937) studied retroactive inhibition in paired-associate learning. The pairs to be learned were made up of Chinese-English words, the Chinese words serving as the stimuli and the English words serving as the responses. Under one condition of this experiment, original learning consisted of learning a list of these Chinese-English pairs while interpolated learning consisted of learning a different set of English words to the same Chinese stimulus words. This fits the A-B, A-K paradigm. Much more inhibition was produced under these circumstances than under a second condition wherein there were no common items in original and interpolated learning (A-B, D-E). It is easy to see that there were, in this second condition, similarities between original and interpolated learning. The first members in both cases were Chinese words and the second members were English words. Both lists were presented, studied, and tested in similar ways. But these similarities are, as one would expect on the hypothesis under discussion, less inhibitory than complete identity of the first members.

The results obtained by Bunch and Winston (1936), demonstrating that amount of inhibition is a function of whether the transfer from the original list to the interpolated learning is positive or negative, bear directly on the transfer theory. Apparently working on the basis of Bruce's (1933) conclusion that learning to make an old response to a new stimulus yields positive transfer, while learn-

ing to make a new response to an old stimulus produces negative transfer, they employed paired nonsense syllables to fit this associative inhibition paradigm. To measure inhibition when transfer had been positive, the original list was XYV-ZOJ with nine other such pairs and the interpolated list was XEH-ZOJ and so on. For negative transfer, the interpolated list was XYV-QUJ and so on. In terms of the symbols used in this section to represent the reproductive inhibition paradigm, positive transfer is given by the interpolation of K-B after A-B. Negative transfer is given by the interpolation of A-K after A-B. Quite aside from its bearing on retroactive inhibition, this experiment corroborates the work of Bruce on transfer (cf. Chap. IX).

The two interpolations, which were introduced just before relearning after an interval of a week, differ substantially in their influence (Table XXXVI). An interpolated list, each pair of which

TABLE XXVI

PERCENTAGES OF RETROACTIVE INHIBITION UNDER TWO DIFFERENT CONDITIONS OF INTERPOLATION

(From Bunch and Winston, *Amer. J. Psychol.*, 1936, 48, p. 604)

Condition	Recall Errors	Relearning Trials	Relearning Errors
A-B : K-B	32.52	60.75	61.55
A-B : A-K	75.75	120.00	148.80

has its first term the same as the first term of an original pair (A-B, A-K), yields much more interference than an interpolated list having second terms common with pairs in the original list (A-B, K-B). Bunch and Winston interpret these results in terms of transfer and cite other studies of similar import.

An hypothesis by E. J. Gibson (1940) applies to the phenomena of verbal learning, including retroactive inhibition, the concepts of generalization and differentiation. On this view, retroactive inhibition results in part from a low discriminability in the material learned. During the learning of an interpolated list, the stimulus items of this list are held to generalize with the stimulus items of

the original list, as a result of which discriminability is lowered and the responses of the original list may occur as intrusions, or there may be other interference of response. Generalization may operate in a similar way to produce a decrement at the time of relearning. The lessened discriminability of the stimuli amounts to a confusion within and between lists. It is important to realize that this method of stating a transfer theory does not change its essential characteristics. As we have seen, generalization is an example of transfer of training, albeit a simple one. Generalization, in turn, is a function of stimulus similarity (cf. Chap. III). Furthermore, discriminability and generalization are measured by very similar operations. The general nature of this approach to the problem of retroaction may be illustrated by one of Gibson's experiments (1941). Her learning materials consisted of linear forms or figures paired with nonsense syllables. The degrees of generalizations of the forms were determined by the technique used earlier by Yum (1931). Judges selected from a number of variant forms the ones which most resembled the standard forms, and then ranked the variations in terms of similarity to the standards. The forms selected were then tested for degree of generalization by having subjects learn a list of paired forms and words and by testing later with the variant forms to discover the number of times a variation was responded to as if it were the standard. When these materials were employed in a retroactive inhibition experiment designed to study amount of inhibition as a function of degree of interlist generalization, the curve in Figure 50 resulted. In a second experiment, both recall and relearning scores show retention to vary inversely as the generalizing tendency of the stimuli in the original and interpolated lists.

These hypotheses concerning retroactive inhibition attribute the decrements which are found in retention to the occurrence of negative transfer. This transfer has its inhibitory effect in terms of competition between different systems of response.[35] It should be noted,

[35] This is the interpretation of inhibition that appears in Guthrie's (1935) book and that Wendt (1936) has also defended. Some of the difficulties involved in accepting retroactive inhibition as a condition for all inhibitory phenomena—for example, conditioned response phenomena—have already been discussed in Chapter II.

however, that the transfer theories do not assume that all of the transfer occurring in the situation is negative. If there is a decrement, negative transfer must have been greater than positive, but both may have occurred, and the outcome may be the algebraic

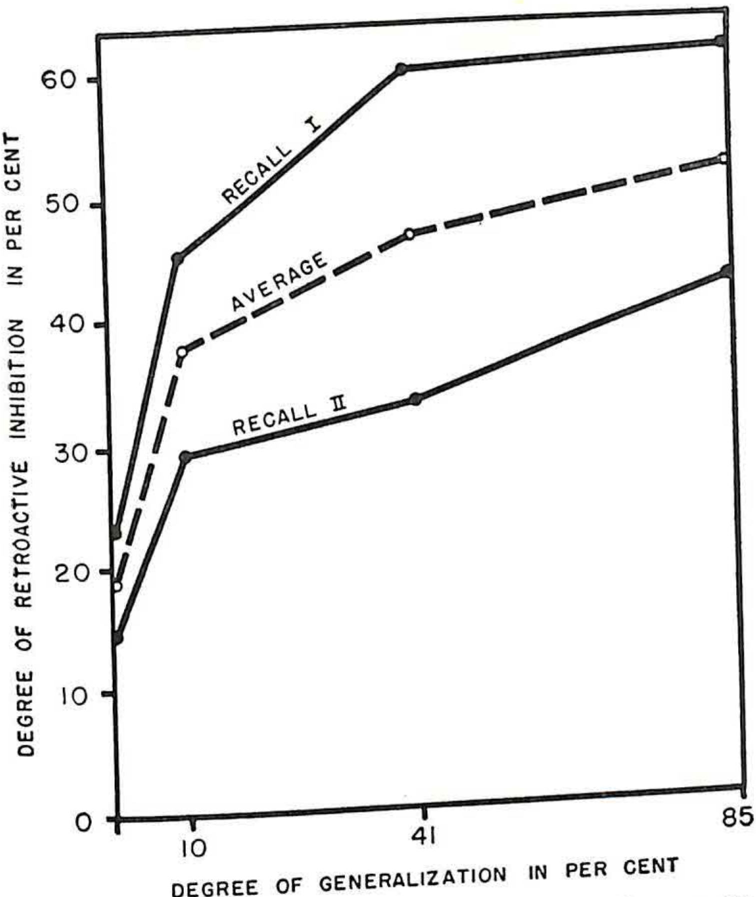

FIG. 50. RETROACTIVE INHIBITION AS A FUNCTION OF DEGREE OF GENERALIZATION

(From E. J. Gibson, *J. exp. Psychol.*, 1941, 28, p. 103)

sum of the two. Thus, relatively unpracticed subjects might acquire considerable increments of learning-how-to-learn from the practice at an interpolated list, and these increments might favor an acceleration of relearning, even though the net effect might be negative.

TWO FACTOR THEORIES

Melton (1940, 1941) has proposed that an unlearning factor, occurring during the learning of the interpolated activity, should be added to competition among responses at the time of relearning. This proposal raises, at once, the question whether the decrement in retention is produced only by the competition between interpolated and original responses at the time of the relearning of the latter, or also during the learning of the interpolated list. McKinney and McGeoch (1935), in their study of overt transfers or intrusions, found somewhat more than three times as many intrusions from the interpolated list to the recall of the original as from the original list to the recall of the interpolated (cf. also Sisson, 1938). This led them to conclude that retroactive inhibition is a two-way interference, with the interpolated-to-original direction dominant under their conditions.

The overt intrusions from the interpolated list to the recall of the original list are not numerous enough, however, to account for all of the retroactive inhibition actually found. Further, Melton and Irwin (1940) hold that the retroactive inhibition not accounted for by overt intrusion is too great to be referred to competition which does not reveal itself in overt intrusions—that is, to implicit intrusions and to competition with no other overt outcome than the blocking of recall. To account for this residual decrement in recall, Melton and his collaborators (1940, 1941) have advanced the hypothesis that there is some other condition, Factor X, operative during interpolated learning to produce the residual decrement. One reasonable possibility is to identify Factor X as an unlearning or experimental extinction of the responses in the original list occurring as intrusions during the learning of the interpolated list. Overt intrusions from the original to the interpolated lists are known to occur, and extinction might follow from the unreinforced occurrence of original responses as errors during interpolated learning.

The residual amount of retroactive inhibition attributable to Factor X is shown in Figure 51, where the data of Melton and Irwin (1940) are plotted. It will be seen that this factor, which from Mel-

ton's discussion we will identify with unlearning, gains in influence as degree of interpolated learning increases, until it has become chiefly responsible for the decrement in recall after forty interpolated trials. The need for Factor X stems from the fact that the curve for retroactive inhibition (in Fig. 49) does not have the same shape as the curve showing the amount of retroactive inhibition attributable to overt competition. No one would expect that all of the

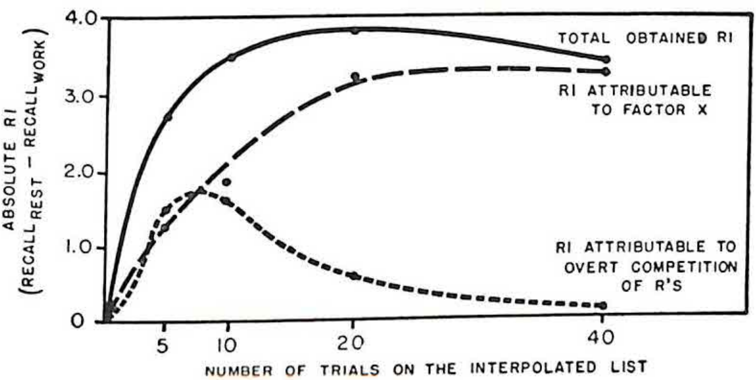

FIG. 51. RELATIONSHIP BETWEEN THE AMOUNT OF RETROACTIVE INHIBITION AND THE DEGREE OF LEARNING OF THE INTERPOLATED MATERIAL

(From Melton and Irwin, *Amer. J. Psychol.*, 1940, 53, p. 198)

The total obtained RI is the number of syllables forgotten as a consequence of the interpolated learning. The RI attributable to the overt competition of original and interpolated responses has been taken as two times the average frequency of intrusions of entire interpolated syllables during the recall trial in order to take into account the unidentified overt intrusions of parts of syllables. The curve for Factor X represents the absolute decrement in recall attributable to the factor or factors other than the overt competition of original and interpolated responses. The materials learned were lists of 18 nonsense syllables; the original lists were given a constant frequency of 5; retention was measured after an interval of 30 minutes.

retroactive inhibition could be accounted for in terms of overt competition, but it is a plausible assumption that overt intrusions ought to account for a constant proportion of the retroactive inhibition. This it does not do, according to Figure 49, and from this discrepancy stems the need for postulating an additional factor.

From the two-factor hypothesis, Melton deduces that the retention of the original activity should be less than that of the inter-

polated. In other words, retroactive inhibition should be greater than proactive inhibition under comparable circumstances. This should be the case because the original activity is subjected both to unlearning during interpolation and to competition during recall and relearning. The interpolated activity, on the other hand, is subjected only to competition. This expectation is fulfilled by the recall scores of the Melton and Von Lackum (1941) experiment where retroactive inhibition is greater than proactive inhibition. A number of studies corroborate this finding (McGeoch and Underwood, 1943; Underwood, 1945, 1948a). On the other hand, this corroborating evidence has not been without its complications. Underwood (1948a) found, for example, that while retroactive inhibition exceeds proactive inhibition when the determination is made five hours after original and interpolated learning, this difference disappears in time, so that, measured after forty-eight hours, there is no difference in proactive and retroactive inhibition. Underwood accounts for this result by assuming that, since the unlearning factor strongly resembles the process of experimental extinction, it also possesses the extinction-like feature of spontaneous recovery.

There are several weaknesses to the unlearning theory which should be made explicit. In the first place, an examination of Figure 49 will reveal that amount of the unlearning factor is supposed to increase as amount of interpolated learning increases. It has been pointed out, however, that the number of intrusions during interpolated learning is small (Thune and Underwood, 1943; Osgood, 1948) and that they tend to be concentrated in the first portion of interpolated learning. There seems to be as little correlation between failure to respond during interpolation and the occurrence of intrusions as is true during relearning (Osgood, 1946, 1948). In the light of this consideration, it is difficult to understand why the amount of unlearning should increase for an appreciable distance during interpolated learning. Also, as Thune and Underwood (1943) have pointed out, there is no particular reason to assume that the ratio between overt and covert intrusions remains constant as amount of interpolated learning increases. If it be

assumed that this ratio varies, then the main necessity for the postulation of Factor X disappears.

This latter assumption, in fact, is the basis of the alternative hypothesis put forth by Underwood (1945). According to this view, the important factor is the degree of differentiation between the competing response systems represented by original and interpolated learning. With low degrees of differentiation, overt intrusions occur. When the systems are differentiated, however, it is as if the subject recognized that the response he was about to give was from the wrong list and so inhibited it. Under the usual rote-learning situation, not enough time remains following this for the subject to give the correct response. Since degree of differentiation is held to be a function of degree of learning (of either or both lists), this is tantamount to saying that the ratio of overt to implicit intrusions changes as degree of interpolated learning increases.

On the basis of his experimental findings, Osgood (1946, 1948) has proposed a somewhat different alternative. He employs the concept of reciprocal inhibition (in a somewhat special sense). That is, when a subject learns one response, he simultaneously learns not to perform opposite responses. In the case of simple motor reactions this is clear. The subject who learns to clench his fist at the sound of a buzzer may also be learning not to open his fist.[36] In the case of verbal learning, a somewhat more elaborate hypothesis becomes necessary. The subject may learn to give the response "white" to a particular stimulus, but he will not be simultaneously learning not to say "black" to that stimulus unless he has previously learned something concerning the continuum of which these two terms form the opposite ends.

[36] That this may not be entirely true is indicated by the results of Wickens (1943a, b, 1945, 1948) on response generalization. Wickens trained his subjects to make a manual response to avoid shock. With the hand turned over, subjects still avoid shock even though an antagonistic muscle group is involved in the reaction (cf., also, McClelland, 1943).

FORGETTING AS A FUNCTION OF ALERTED STIMULATING CONDITIONS

The second fundamental condition in the current psychological account of forgetting is an alteration of the stimulating conditions from the time of learning to that of the measurement of retention. Such alteration produces a decrement in retention if, as a result of it (a) the stimuli necessary to elicit the originally learned acts are not effectively present or (b) new stimuli are introduced which evoke competing responses in sufficient strength to block the originally learned ones. In a strict sense, behavioral changes which occur under such conditions should not be termed forgetting. Learned responses are elicited by stimuli and it is not reasonable to suppose that a habit should manifest itself in behavior when the eliciting stimuli are not present. In other words, the term "forgetting" implies that the stimulating conditions are the same during learning and the measurement of retention. On the other hand, it is never possible to repeat stimulus situations exactly, and in the everyday situations of remembering, variations in stimulus context undoubtedly play a considerable role in determining the course of retention.

Everything learned is learned in response to stimulating or antecedent conditions which are a part of the learning situation and which are specific to it. Learning also occurs in a complex context of environing conditions not specific to the particular acts being learned. There is the obvious external environment of stimuli impinging on the individual's exteroceptors, and there is the less obvious environment of intraorganic stimuli to the interoceptors. Correlated with these is the context of the individual's symbolic or ideational events. All are incidentally but inevitably present during practice, and it follows that the activities learned should also be associated with some features of these environments.

Recall or any other measure of retention depends upon the presence of some stimulating situation with which the learned act is directly or indirectly associated. It is to be expected that, other things being equal, retention will be higher the more numerous and complete are the stimuli earlier associated with the activity and

now present again at the time of measurement of retention (Carr, 1925). Alteration or removal of these stimuli at the time of recall will be correlated with a failure of recall.

The work of Yum (1931), already described in Chapter IX, is an excellent case in point. It will be remembered that every alteration of the stimulus member of paired nonsense syllables reduced recall by a statistically significant amount. When paired meaningful words or paired visual patterns were used, introduction at recall of other words or patterns of known degree of similarity to the original stimulus reduced recall, the amount of reduction being greater the less the similarity.

This experiment illustrates both the influence of altered stimulating conditions on amount recalled and the identity of experiments on altered context and on transfer. Experiments on one are also experiments on the other. Transfer experiments measure retention under conditions which differ more from those of learning than do those which employ the other methods of measuring retention, and the introduction of these differences is an introduction of changed stimulation. The fact that positive transfer is nearly always less than 100 per cent is a demonstration that altered stimulation may reduce retention. Yum's work, and that of the other experimenters cited with him in Chapter IX, may be regarded from two points of view. Yum's experiment, regarded as an experiment on transfer, measured the amount of transfer from one stimulating condition to another changed in a specified manner. Regarded as an experiment on retention as a function of altered context, Yum's results demonstrate that decrements in recall follow such a change.

Experiments on transfer deal with changes from the material practiced to the material tested. The alterations made are thus specific to the activities employed. But a subject associates with these activities other stimulating conditions than those specific to the activities themselves. Some aspects of the external environment also get associated with activities learned, and alteration of these aspects in certain ways at the time of recall produces a decrement. Pan's (1926) study of the influence of verbal context was the first systematic attack on this problem with human subjects. Previous

work had shown that alterations of the environment in which rats had learned a maze brought about decrements in performance (Carr, 1917). Pan's data show an intimate relation between the recall of paired associates and the presence of incidental contextual words. Removal during recall of a context logically related to the response word and present throughout learning decreased recall. On the other hand, if the context had been varied during learning, this effect was diminished. Change from an old context to a new one at recall yielded a decrement, but introduction, during recall, of a context logically related to the response word increased recall.

When a list of paired syllables is presented against one background of color for learning and against a different one for relearning, the amount and accuracy of recall are diminished and the trials to relearn are increased (Dulsky, 1935). This effect is greater when the alteration is in the background of the response word. Where practice is done without an audience, the presence of a small audience during relearning decreases retention (Burri, 1931), as does changing the sensory mode of presentation from auditory to visual or the reverse (Reed, 1931). When classroom learning is later tested in a different classroom, or with a different proctor for the test, recall is diminished. The diminution is somewhat greater when both classroom and proctor are changed (Abernethy, 1940). The implications of laboratory experimentation are here corroborated under classroom conditions. These results should not be interpreted to mean that every alteration of the environment will yield a decrement. Not all aspects of the context are associated with the activity practiced, and an alteration of the unassociated or weakly associated aspects obviously will not affect the activity.[37]

From observation of individual cases come a number of illustrations of diminished retention as a function of altered environmental context, of which the following employed by Carr (1925) is one:

"An individual lived for several years in China and laboriously acquired the ability to speak the Chinese language. Upon his return to this country

[37] For illustrations of altered external context without decrement in retention, cf. Reed (1931), Pessin (1932), and Farnsworth (1934, 1937).

for a couple of years' vacation he found to his dismay that his ability to speak and understand this language had practically disappeared by the end of this time. Naturally he expected that a considerable amount of effort and time would be required to relearn the language, but to his surprise he found that he was able to speak the language quite fluently upon his return to China."

Carr cites to the same end cases of inability to recall memories of early youth when the effort to recall is made away from the original environment and after long absence from it, followed by a ready return of memories when one enters the familiar environment again.

A similar case has been reported by Waters (1934). A five-year-old girl lost the use of English after a two-week visit in Holland, during which time she learned Dutch. She had heard Dutch at home, could probably understand it, but had not used it. In approximately the same time, after her return to an English-speaking environment, she relearned English. During her visit to Holland, although the use of English had been functionally lost, not all traces of it had gone, for she spoke Dutch with an English accent and constructed sentences according to English syntax. It is a reasonable interpretation that the decrement in retention was a function of the stimulating context.

FORGETTING AS A FUNCTION OF INADEQUATE SET

The role of set in retention is intimately related to the warming-up phenomenon, some aspects of which we have already discussed in connection with transfer of training (Chap. IX) and other topics. It will be remembered that, if the learner performs, before learning, some activity which is similar to the learning activity, speed of learning is increased (Thune, 1950a). This effect is a function of the amount of time devoted to the warming-up activity. The beneficial effects tend to dissipate rapidly in time according to the findings of Hamilton (1950). A considerable number of studies have shown the warming-up effect to exist in a relearning curve, where its presence is revealed by the steeper slope of the relearning curve than of the original learning curve at the same initial level of per-

formance.[38] In such cases, the relearning trials serve not only to afford additional practice in the sense of habit acquisition, but also to warm up the learner, that is, to induce in him the appropriate performance set. This finding suggests that initial relearning performance (recall) may be, in part, a function of the subject's need for warming up (cf. Ammons, 1947). Following Ammons' formulations, Irion (1948) applied the concepts of warming up and set to retention. Under this hypothesis, during learning the subject acquires particular perceptual and motor adjustments which facilitate the performance. These are lost during the retention interval, presumably because intervening activity demands the assumption of other, incompatible, sets. At the time of recall and relearning, then, the subject must reacquire the appropriate performance set. The period during which this resumption of set takes place is characterized by the rapid improvement of performance, or what we have called the warming-up effect. From these considerations, and from the results obtained by Thune (1950) and Hamilton (1950), we would be led to the conclusion that a period of warming up introduced before recall should improve retention. It goes without saying, of course, that the warming-up activity which is used should be one which does not afford the subject practice on the habits in question. Thus, color-naming, color-guessing, and digit-naming have been used as warming-up activities for verbal rote learning. The colors or digits to be named are presented to the subject on the memory drum at the same rate as the words or syllables to be learned. The subject is required to vocalize rhythmically, just as he is in the rote-learning situation. The warming-up activity, then, should be one which requires the subject to engage in the same kinds of activities that are involved in learning, but which should not give him practice on the habits to be learned.

The amount of benefit to be derived from engaging in a warming-up task before recall is probably a function of three vari-

[38] It will be remembered that Luh (1922) found that the retention curve determined by the saving method does not fall as rapidly as the retention curve determined by measuring recall. This, of course, is another way of noting that the slope of the relearning curve, and hence amount of warming up, is a function of the length of the retention interval.

ables or conditions: (a) the amount of the warming-up activity, (b) the similarity of the behaviors involved in the warming-up activity and the learning activity, and (c) the time interval between the warming-up activity and the recall.

Several experiments have been conducted concerning these possibilities. Irion (1949a) demonstrated the beneficial effects of a warming-up task introduced before recall. Using paired-associate learning with a 24-hour retention interval, he found that approximately 87 per cent of the forgetting which occurred over this interval was removed by giving the subject a single trial of color-naming, the colors being presented on the memory drum.[39] Two additional studies have considered retention as a function of amount of pre-recall warming up. Using serial learning of nonsense syllables to a partial criterion and a 35-minute retention interval, Irion and Wham (1951) used digit-naming as a warming-up activity. In this experiment, 0, 0.5, 1, 2, and 4 warming-up trials were given. The results are shown in Table XXVII. It will be seen that there is a fairly orderly relationship between amount of warming up and performance on the first relearning trial. In a similar experiment, Irion (1949b) used a 48-hour retention interval and up to 10 trials of warming up. His results indicated that, as amount of warming up increases, level of recall at first increases and then decreases. In this case, the maximum recovery from forgetting (77 per cent) occurred following five trials of pre-recall warming up. Since Thune (1950a) found continuing benefits from warming up before original learning up to 10 trials of warming up, and since this has also been shown in the retention situation by Hartley (1948), it is probable that this inversion in the relationship is due to the fact that Irion's (1949b) study employed serial anticipation learning while the studies of Thune and Hartley employed paired-associate learning. Since rate of responding is higher in serial learning than in paired-associate learning, the implication seems clear than Irion's findings

[39] This result has failed of corroboration in a study by Rockway (1951). Rockway's findings are discussed in a later paragraph. Indirect corroboration is found in Ward's (1937) monograph on reminiscence. In this case, reliably greater reminiscence was obtained when the subjects named colors during the interval (a warming-up activity) than when they engaged in light reading.

result from his greater massing of the warming-up activity. With the exception of Rockway's (1951) results, then, there appears to be a fairly clear, increasing relationship between amount of warming up and level of recall.

TABLE XXVII

THE RELATIONSHIP BETWEEN LEVEL OF RECALL AND AMOUNT OF PRE-RECALL WARMING UP

(From Irion and Wham, *J. exp. Psychol.*, in press)

Condition	Mean Number of Correct Anticipations on Last Original Learning Trial	Mean Number of Correct Anticipations on First Relearning Trial	Difference
I (No rest)	6.00	6.00	0.00
II (No warming up, 35 min. rest)	6.07	4.40	−0.33
III (0.5 trial warming up, 35 min. rest)	6.00	5.67	−1.62
IV (1 trial warming up, 35 min. rest)	6.20	5.46	−0.74
V (2 trials warming up, 35 min. rest)	5.87	5.53	−0.34
VI (4 trials warming up, 35 min. rest)	4.53	5.87	+1.34

With regard to similarity, little information exists. Since all of the exploratory studies on the warming-up effect have been designed to obtain that effect, warming-up activities have been selected which were as similar as possible to the learning activity. Rockway's (1951) experiment carefully investigated the relationship between warming-up effects and the amount of similarity between the warming up and the learning activities. Unfortunately, Rockway failed to obtain a beneficial effect from warming up, even under conditions of maximum similarity.

The time interval between warming up and retention has not

been thoroughly investigated. Hartley (1948) had subjects partially learn a list of paired-associate adjectives and then relearn them after 24 hours. In one condition of his experiment, 10 warming-up trials were given immediately following original learning. In a second condition, these warming-up trials were given immediately before recall. In the former case there was an 11 per cent recovery from retention loss, while in the latter case there was a 90 per cent recovery. Presumably, then, if the intervening points were to be investigated, a function resembling the one obtained by Hamilton (1950) would be found.

It is difficult to interpret the findings on the warming-up effect as these apply to retention. At first glance, it would appear that the large reductions in forgetting which are obtained point to the conclusion that this factor is an extremely important one, at least in the explanation of "everyday" forgetting. It would appear, further, that the explanation of forgetting in terms of retroactive inhibition is distinctly limited and that losses due to retroaction occur only under conditions where there is a formal interpolation of material which transfers negatively to the recall of the original. Such a conclusion would be premature. In the first place, it would be desirable to know whether or not the warming-up effect can be found in the recall of retroactively inhibited material. Such an experiment has not yet been conducted. In the second place, the data on the warming-up effect are much too scanty to derive conclusions regarding the generality of the effect. Lastly, Rockway's (1951) failure to obtain warming-up effects in retention points to the need for a cautious interpretation. Although the reason for Rockway's failure to obtain these effects is not clear at the present time, it is certain that his findings place some restrictions upon the generality of an explanation of forgetting in terms of a need for warming the subject up to a proper performance set.

WHY IS FORGETTING NOT COMPLETE?

The conditions which are basic to forgetting are so ubiquitous in daily life as to raise the question of why one remembers anything. Each succeeding activity is an interpolation to all that have gone

before, so that intervening activity progressively accumulates. Stimulating contexts and sets are constantly changing. Why, then, does one remember anything at all?

The situation is not as difficult as it sounds. There is often a minimal similarity between the original and interpolated activities of daily life, and even among the activities we formally learn. Further, the major portion of what we wish to recall was meaningful at the time of acquisition and was knit in with much else in our already acquired and retained organization. Relatively meaningful material is less susceptible to interference than is less meaningful material because of the support it receives from the transferred effects of other habits. By virtue of its complex connections, moreover, meaningful material may be elicited by any one of many different stimuli and related sets.

Much of what one learns is relatively well learned, if not initially, then by distributed practice and rehearsal, and is, therefore, less susceptible to a decrement from intervening activity, a great deal of which is often not well fixated and relatively dissimilar to what has gone before. Unwittingly, our sets and other conditions isolate materials from each other, thereby reducing interference, and when interference does occur it is relatively transitory. Add to that the subtlety of the human being's symbolic function by means of which sets may be reinstated by manifold cues, and the conditions producing forgetting may be, and are surprisingly well, overcome.

REFERENCES

Abel, T. M. Neuro-circulatory reaction and the recall of unfinished and completed tasks. *J. Psychol.*, 1938, 6, 377-383.

Abernethy, E. M. The effect of changed environmental conditions upon the results of college examinations. *J. Psychol.*, 1940, 10, 293-301.

Achilles, E. M. Experimental studies in recall and recognition. *Arch. Psychol., N. Y.*, 1920, 6, No. 44.

Allport, G. W. Change and decay in the visual memory image. *Brit. J. Psychol.*, 1930, 21, 133-148.

Alper, T. G. Memory for completed and incompleted tasks as a function of personality: an analysis of group data. *J. abnorm. soc. Psychol.*, 1946, 41, 403-421.

Ammons, R. B. Acquisition of motor skill. I. Quantitative analysis and theoretical formulation. *Psychol. Rev.*, 1947, 54, 263-281.

Anastasi, A. Further studies on the memory factor. *Arch. Psychol., N. Y.*, 1932, 22, No. 142.

Andrew, D. M., and Bird, C. A comparison of two new-type questions: recall and recognition. *J. educ. Psychol.*, 1938, 29, 175-193.

Austin, S. D. M. A study in logical memory. *Amer. J. Psychol*, 1921, 32, 370-403.

Ballard, P. B. Obliviscence and reminiscence. *Brit. J. Psychol., Monogr. Suppl.*, 1913, 1, No. 2.

Barrett, D. M. Memory in relation to hedonic tone. *Arch. Psychol., N. Y.*, 1938, 31, No. 223.

Bartlett, F. C. *Remembering: a study in experimental and social psychology.* New York: Macmillan; Cambridge, England: Univ. Press, 1932.

Bassett, S. J. Retention of history in the sixth, seventh and eight grades with special reference to the factors that influence retention. *Johns Hopk. Stud. Educ.*, 1928, No. 12.

Bean, C. H. The curve of forgetting. *Arch. Psychol., N. Y.*, 1912, 3, No. 21.

Beebe-Center, J. G. *The psychology of pleasantness and unpleasantness.* New York: Van Nostrand, 1932.

Bell, H. M. Retention of pursuit rotor skill after one year. *J. exp. Psychol.*, 1950, 40, 648-649.

Blankenship, A. B., and Whitely, P. L. Proactive inhibition in the recall of advertising material. *J. soc. Psychol.*, 1941, 13, 311-322.

Boguslavsky, G. W., and Guthrie, E. R. The recall of completed and interrupted activities: an investigation of Zeigarnik's experiment. *Psychol. Bull.*, 1941, 38, 575-576. (Abstract.)

Boring, E. G. Communality in relation to proaction and retroaction. *Amer. J. Psychol.*, 1941, 54, 280-283.

Braun, H. W., Russell, R. W., and Patton, R. A. Duration of effects of electroshock convulsions on retention of a maze habit in white rats. *J. comp. physiol. Psychol.*, 1949, 42, 332-337.

Briggs, L. J., and Reed, H. B. The curve of retention for substance material. *J. exp. Psychol.*, 1943, 32, 513-517.

Britt, S. H. Retroactive inhibition: a review of the literature. *Psychol. Bull.*, 1935, 32, 381-440.

Britt, S. H. Theories of retroactive inhibition. *Psychol. Rev.*, 1936, 43, 207-216.

Britt, S. H., and Bunch, M. E. Jost's law and retroactive inhibition. *Amer. J. Psychol.*, 1934, 46, 299-308.

Brown, W. To what extent is memory measured by a single recall? *J. exp. Psychol.*, 1923, 6, 377-382.

Brown, W. Growth of "memory images." *Amer. J. Psychol.*, 1935, 47, 90-102.

Bruce, R. W. Conditions of transfer of training. *J. exp. Psychol.*, 1933, 16, 343-361.

Bugelski, B. R. Interference with recall of original responses after learning new responses to old stimuli. *J. exp. Psychol.*, 1942, 30, 368-379.

Bunch, M. E. The amount of transfer in rational learning as a function of time. *J. comp. Psychol.*, 1936, 22, 325-337.

Bunch, M. E. The measurement of retention by the relearning method. *Psychol. Rev.*, 1941, 48, 450-456.

Bunch, M. E., and McTeer, F. D. The influence of punishment during learning upon retroactive inhibition. *J. exp. Psychol.*, 1932, 15, 473-495.

Bunch, M. E., and Winston, M. M. The relationship between the character of the transfer and retroactive inhibition. *Amer. J. Psychol.*, 1936, 48, 598-608.

Burri, C. The influence of an audience upon recall. *J. educ. Psychol.*, 1931, 22, 683-690.

Burton, A., and Tueller, R. Successive reproductions of visually perceived forms. *J. genet. Psychol.*, 1941, 58, 71-82.

Burtt, H. E., and Dobell, E. M. The curve of forgetting for advertising material. *J. appl. Psychol.*, 1925, 9, 5-21.

Buxton, C. E., and Newman, E. B. The forgetting of 'crowded' and 'isolated' materials. *J. exp. Psychol.*, 1940, 26, 180-198.

Cain, L. F., and Willey, R. de V. The effect of spaced learning on the curve of retention. *J. exp. Psychol.*, 1939, 25, 209-214.

Carr, H. A. Maze studies with the white rat. I. Normal animals; II. Blind animals; III. Anosmic animals. *J. Anim. Behav.*, 1917, 7, 259-275; 277-294; 295-306.

Carr, H. A. *Psychology: a study of mental activity.* New York: Longmans, Green, 1925.

Cason, H. The learning and retention of pleasant and unpleasant activities. *Arch. Psychol., N. Y.*, 1932, 21, No. 134.

Cheng, N. Y. Retroactive effect and degree of similarity. *J. exp. Psychol*, 1929, 12, 444-449.

Clark, B. E. The effect upon retention of varying lengths of study periods and rest intervals in distributed learning time. *J. educ. Psychol.*, 1928, 19, 552-559.

Clark, K. B. Some factors influencing the remembering of prose material. *Arch. Psychol., N. Y.*, 1940, 35, No. 253.

Clarke, H. M. Recall and recognition for faces and names. *J. appl. Psychol.*, 1934, 18, 757-763.

Crosland, H. R. A qualitative analysis of the process of forgetting. *Psychol. Monogr.*, 1921, 29, No. 130.

Dietze, A. G., and Jones, G. E. Factual memory of secondary school pupils for a short article which they read a single time. *J. educ. Psychol.*, 1931, 22, 586-598; 667-676.

Dreis, T. A. Two studies in retroaction. I. Influence of partial identity. II. Susceptibility to retroaction at various grade levels. *J. gen. Psychol.*, 1933, 8, 157-171.

Dudycha, G. J., and Dudycha, M. M. Some factors and characteristics of childhood memories. *Child Develpm.*, 1933, 4, 265-278.

Dudycha, G. J., and Dudycha, M. M. Childhood memories: a review of the literature. *Psychol. Bull.*, 1941, 38, 668-682.

Dulsky, S. G. The effect of a change of background on recall and relearning. *J. exp. Psychol.*, 1935, 18, 725-740.

Duncan, C. P. The effect of electroshock convulsions on the maze habit in the white rat. *J. exp. Psychol.*, 1945, 35, 267-278.

Duncan, C. P. Habit reversal induced by electro-shock in the rat. *J. comp. physiol. Psychol.*, 1948, 41, 11-16.

Duncan, C. P. The retroactive effect of electroshock on learning. *J. comp. physiol. Psychol.*, 1949, 42, 32-44.

Ebbinghaus, H. *Das Gedächtnis: Untersuchungen zur experimentellen Psychologie.* Leipzig: Duncker and Humblot, 1885. (Trans. as *Memory: a contribution to experimental psychology* by H. A. Ruger and C. E. Bussenius. New York: Teachers College, Columbia Univ., 1913).

Ebbinghaus, H. *Grundzüge der Psychologie.* (3rd Ed.) Leipzig: Veit, 1911. Vol. I, p. 685.

Edwards, A. L. The retention of affective experiences; a criticism and restatement of the problem. *Psychol. Rev.*, 1942, 49, 43-53.
Farnsworth, P. R. Examinations in familiar and unfamiliar surroundings. *J. soc. Psychol.*, 1934, 5, 128-129.
Farnsworth, P. R. Continued training with the omission of certain Nebenreize. *J. genet. Psychol.*, 1937, 50, 227-282.
Finger, F. W. Convulsive behavior in the rat. *Psychol. Bull.*, 1947, 44, 201-248.
Finkenbinder, E. O. The curve of forgetting. *Amer. J. Psychol.*, 1913, 24, 8-32.
Flügel, J. C. A quantitative study of feeling and emotion in everyday life. *Brit. J. Psychol.*, 1925, 15, 318-355.
Frank, J. D., and Ludvigh, E. J. The retroactive effect of pleasant and unpleasant odors on learning. *Amer. J. Psychol.*, 1931, 43, 102-108.
Freeman, F. N., and Abernethy, E. M. Comparative retention of typewriting and of substitution with analogous material. *J. educ. Psychol.*, 1930, 21, 639-647.
Freeman, F. N., and Abernethy, E. M. New evidence of the superior retention of typewriting to that of substitution. *J. educ. Psychol.*, 1932, 23, 331-334.
Freeman, G. L. Changes in tonus during completed and interrupted mental work. *J. gen. Psychol.*, 1930, 4, 309-333.
French, J. W. The effect of temperature on the retention of a maze habit in fish. *J. exp. Psychol.*, 1942, 31, 79-87.
Freud, S. *Psychopathology of everyday life.* New York: Macmillan, 1914.
Freud, S. *Collected papers.* London: Hogarth, 1925. Vol. IV.
Gibson, E. J. A systematic application of the concepts of generalization and differentiation to verbal learning. *Psychol. Rev.*, 1940, 47, 196-229.
Gibson, E. J. Retroactive inhibition as a function of degree of generalization between tasks. *J. exp. Psychol.*, 1941, 28, 93-115.
Gibson, E. J. Intra-list generalization as a factor in verbal learning. *J. exp. Psychol.*, 1942, 30, 185-200.
Gibson, E. J., and Gibson, J. J. Retention and the interpolated task. *Amer. J. Psychol.*, 1934, 46, 603-610.
Gibson, J. J. The reproduction of visually perceived forms. *J. exp. Psychol.*, 1929, 12, 1-39.
Gibson, J. J. Retroaction and the method of recognition. *J. gen. Psychol.*, 1934, 10, 234-236.
Gilbert, G. M. The age difference in the hedonistic tendency in memory. *J. exp. Psychol.*, 1937, 21, 433-441.
Gilbert, G. M. The new status of experimental studies on the relationship of feeling to memory. *Psychol. Bull.*, 1938, 35, 26-35.
Gillette, A. L. Learning and retention: a comparison of three experimental procedures. *Arch. Psychol., N. Y.*, 1936, 28, No. 198.
Glixman, A. F. Recall of completed and incompleted activities under varying degrees of stress. *J. exp. Psychol.*, 1949, 39, 281-295.
Goldmeier, E. Progressive changes in memory traces. *Amer. J. Psychol.*, 1941, 54, 490-503.
Gordon, K. Class results with spaced and unspaced memorizing. *J. exp. Psychol.*, 1925, 8, 337-343.
Gordon, K. A study of early memories. *J. Delinqu.* 1928, 12, 129-132.
Guilford, J. P., and Park, D. G. The effect of interpolated weights upon comparative judgments. *Amer. J. Psychol.*, 1931, 43, 589-599.
Guthrie, E. R. *The psychology of learning.* New York: Harper, 1935.

Haagen, C. H. Learning and retention as a function of the synonymity of original and interpolated tasks. Ph.D. Thesis, State Univ. Iowa, 1943.

Hall, V. The effects of time interval on recall. *Brit. J. Psychol.*, 1936, 27, 41-50.

Hamilton, C. E. The relationship between length of interval separating two learning tasks and performance on the second task. *J. exp. Psychol.*, 1950, 40, 613-621.

Hanawalt, N. G. Memory trace for figures in recall and recognition. *Arch. Psychol., N. Y.*, 1937, 31, No. 216.

Harden, L. M. The quantitative study of the similarity factor in retroactive inhibition. *J. gen. Psychol.*, 1929, 2, 421-430.

Harden, L. M. The effect of emotional reactions upon retention. *J. gen. Psychol.*, 1930, 3, 197-220.

Harrower, M. R. Organization in higher mental processes. *Psychol. Forsch.*, 1932, 17, 56-120.

Hartley, T. C. Retention as a function of the temporal position of an interpolated warming-up task. M. A. Thesis, Univ. of Ill., 1948.

Heine, R. Über Wiedererkennen und rückwirkende Hemmung. *Z. Psychol.*, 1914, 68, 161-236.

Hilgard, E. R., and Campbell, A. A. The course of acquisition and retention of conditioned eyelid responses in man. *J. exp. Psychol.*, 1936, 19, 227-247.

Hill, L. B., Rejall, A. E., and Thorndike, E. L. Practice in the case of typewriting. *Ped. Sem.*, 1913, 20, 516-529.

Hoagland, H. A study of the physiology of learning in ants. *J. gen. Psychol.*, 1931, 5, 21-41.

Horowitz, M. W., and Stone, C. P. The disorganizing effects of electro-convulsive shocks on a light discrimination habit in albino rats. *J. comp. physiol. Psychol.*, 1947, 40, 15-21.

Hovland, C. I. Experimental studies in rote-learning theory. I. Reminiscence following learning by massed and by distributed practice. *J. exp. Psychol.*, 1938a, 22, 201-224.

Hovland, C. I. Experimental studies in rote-learning theory. II. Reminiscence with varying speeds of syllable presentation. *J. exp. Psychol.*, 1938b, 22, 338-353.

Hovland, C. I. Experimental studies in rote-learning theory. VI. Comparison of retention following learning to the same criterion by massed and distributed practice. *J. exp. Psychol.*, 1940, 26, 568-587.

Hughes, R. A. A direct measure of response generalization. M. A. Thesis, Northwestern Univ., 1949.

Hull, C. L. *Hypnosis and suggestibility: an experimental approach.* New York: Appleton-Century, 1933.

Hunter, W. S. The effect of inactivity produced by cold upon learning and retention in the cockroach, *blatella germanica. J. genet. Psychol.*, 1932, 41, 253-265.

Irion, A. L. Retroactive inhibition as a function of the relative serial positions of the original and interpolated items. *J. exp. Psychol.*, 1946, 36, 262-270.

Irion, A. L. The relation of 'set' to retention. *Psychol. Rev.*, 1948, 55, 336-341.

Irion, A. L. Retention and warming-up effects in paired-associate learning. *J. exp. Psychol.*, 1949a, 39, 669-675.

Irion, A. L. Retention as a function of amount of pre-recall warming up. *Amer. Psychologist*, 1949b, 4, 219-220. (Abstract.)

Irion, A. L., and Wham, D. S. Recovery from retention loss as a function of amount of pre-recall warming up. *J. exp. Psychol.*, 1951, 41, 242-246.

Irwin, F. W., and Seidenfeld, M. A. The application of the method of comparison to the problem of memory changes. *J. exp. Psychol.*, 1937, 21, 363-381.

James, W. *The principles of psychology.* New York: Holt, 1890.

Jenkin, A. M. Imagery and learning. *Brit. J. Psychol.*, 1935, 26, 149-164.

Jenkins, J. G., and Dallenbach, K. M. Obliviscence during sleep and waking. *Amer. J. Psychol.*, 1924, 35, 605-612.

Jenkins, W. O., and Postman, L. An experimental analysis of set in rote learning: retroactive inhibition as a function of changing set. *J. exp. Psychol.*, 1949, 39, 69-73.

Johnson, L. M. Similarity of meaning as a factor in retroactive inhibition. *J. gen. Psychol.*, 1933, 9, 377-388.

Katona, G. *Organizing and memorizing: studies in the psychology of learning and teaching.* New York: Columbia Univ. Press, 1940.

Kennelly, T. W. The role of similarity in retroactive inhibition. *Arch. Psychol., N. Y.*, 1941, 37, No. 260.

Krueger, W. C. F. The effect of overlearning on retention. *J. exp. Psychol.*, 1929, 12, 71-78.

Kreuger, W. C. F. Further studies in overlearning. *J. exp. Psychol.*, 1930, 13, 152-163.

Kuhlmann, F. On the analysis of the memory consciousness: a study in the mental imagery and memory of meaningless visual forms. *Psychol. Rev.*, 1906, 13, 316-348.

Lahey, M. F. L. Retroactive inhibition as a function of age, intelligence, and the duration of the interpolated activity. *Cath. Univ. Amer. educ. Res. Monogr.*, 1937, 10, No. 2.

Lauenstein, O. Ansatz zu einer physiologischen Theorie des Vergleichs und der Zeitfehler. *Psychol. Forsch.*, 1933, 17, 130-177.

Leavitt, H. J., and Schlosberg, H. The retention of verbal and motor skills. *J. exp. Psychol.*, 1944, 34, 404-417.

Lester, O. P. Mental set in relation to retroactive inhibition. *J. exp. Psychol.*, 1932, 15, 681-699.

Lewin, K. *A dynamic theory of personality.* (Trans. by D. K. Adams and K. E. Zener.) New York: McGraw-Hill, 1935.

Lissner, K. Die Entspannung von Bedürfnissen durch Ersatzhandlungen. *Psychol. Forsch.*, 1933, 18, 218-250.

Luh, C. W. The conditions of retention. *Psychol. Monogr.*, 1922, 31, No. 142.

Marrow, A. J. Goal tensions and recall. I, II. *J. gen. Psychol.*, 1938, 19, 3-35; 37-64.

Martin, J. R. Reminiscence and gestalt theory. *Psychol. Monogr.*, 1940, 52, No. 235.

McClelland, D. C. Studies in serial verbal discrimination learning. IV. Habit reversal after two degrees of learning. *J. exp. Psychol.*, 1943, 33, 457-470.

McGeoch, J. A. The influence of degree of learning upon retroactive inhibition. *Amer. J. Psychol.*, 1929, 41, 252-262.

McGeoch, J. A. The influence of four different interpolated activities upon retention. *J. exp. Psychol.*, 1931, 14, 400-413.

McGeoch, J. A. Forgetting and the law of disuse. *Psychol. Rev.*, 1932a, 39, 352-370.

McGeoch, J. A. The influence of degree of interpolated learning upon retroactive inhibition. *Amer. J. Psychol.*, 1932b, 44, 695-708.

McGeoch, J. A. The comparative retention values of a maze habit, of nonsense syllables, and of rational learning. *J. exp. Psychol.*, 1932c, 15, 662-680.

McGeoch, J. A. Studies in retroactive inhibition. VII. Retroactive inhibition as a function of the length and frequency of presentation of the interpolated lists. *J. exp. Psychol.*, 1936, 19, 674-693.

McGeoch, J. A., and McDonald, W. T. Meaningful relation and retroactive inhibition. *Amer. J. Psychol.*, 1931, 43, 579-588.

McGeoch, J. A., and McGeoch, G. O. Studies in retroactive inhibition. VI. The influence of the relative serial positions of interpolated synonyms. *J. exp. Psychol.*, 1936, 19, 1-23.

McGeoch, J. A., and McKinney, F. Retroactive inhibition in the learning of poetry. *Amer. J. Psychol.*, 1934a, 46, 19-33.

McGeoch, J. A., and McKinney, F. The susceptibility of prose to retroactive inhibition. *Amer. J. Psychol.*, 1934b, 46, 429-436.

McGeoch, J. A., McKinney, F., and Peters, H. N. Studies in retroactive inhibition. IX. Retroactive inhibition, reproductive inhibition and reminiscence. *J. exp. Psychol.*, 1937, 20, 131-143.

McGeoch, J. A., and Melton, A. W. The comparative retention values of maze habits and of nonsense syllables. *J. exp. Psychol.*, 1929, 12, 392-414.

McGeoch, J. A., and Underwood, B. J. Tests of the two-factor theory of retroactive inhibition. *J. exp. Psychol.*, 1943, 32, 1-16.

McGeoch, J. A., and Whitely, P. L. The recall of observed material. *J. educ. Psychol.*, 1926, 17, 419-425.

McKinney, F. Studies in the retention of interrupted learning activities. *J. comp. Psychol.*, 1935a, 19, 265-296.

McKinney, F. Retroactive inhibition and recognition memory. *J. exp. Psychol.*, 1935b, 18, 585-598.

McKinney, F., and McGeoch, J. A. The character and extent of transfer in retroactive inhibition: disparate serial lists. *Amer. J. Psychol.*, 1935, 47, 409-423.

Meek, L. H. A study of learning and retention in young children. *Teach. Coll. Contr. Educ.*, 1925, No. 164.

Melton, A. W. Review of Katona, *Organizing and memorizing*. *Amer. J. Psychol.*, 1941, 54, 455-457.

Melton, A. W., and Irwin, J. McQ. The influence of degree of interpolated learning on retroactive inhibition and the overt transfer of specific responses. *Amer. J. Psychol.*, 1940, 53, 173-203.

Melton, A. W., and Von Lackum, W. J. Retroactive and proactive inhibition in retention: evidence for a two-factor theory of retroactive inhibition. *Amer. J. Psychol.*, 1941, 54, 157-173.

Meltzer, H. The present status of experimental studies on the relationship of feeling to memory. *Psychol. Rev.*, 1930, 37, 124-139.

Minami, H., and Dallenbach, K. M. The effect of activity upon learning and retention in the cockroach. *Amer. J. Psychol.*, 1946, 59, 1-58.

Morgan, R. L., and Underwood, B. J. Proactive inhibition as a function of response similarity. *J. exp. Psychol.*, 1950, 40, 592-604.

Morris, W. W. Story remembering among children. *J. soc. Psychol.*, 1939, 10, 489-502.

Müller, G. E., and Pilzecker, A. Experimentelle Beiträge zur Lehre vom Gedächtniss. *Z. Psychol.*, 1900, Erbd. 1, 1-300.

Nagge, J. W. An experimental test of the theory of associative interference. *J. exp. Psychol.*, 1935, 18, 663-682.

Newman, E. B. Forgetting of meaningful material during sleep and waking. *Amer. J. Psychol.*, 1939, 52, 65-71.

Nowlis, H. H. The influence of success and failure on the resumption of an interrupted task. *J. exp. Psychol.*, 1941, 28, 304-325.

Osgood, C. E. Meaningful similarity and interference in learning. *J. exp. Psychol.*, 1946, 36, 277-301.

Osgood, C. E. An investigation into the causes of retroactive interference. *J. exp. Psychol.*, 1946, 36, 277-301.

Osgood, C. E. The similarity paradox in human learning: a resolution. *Psychol. Rev.*, 1949, 56, 132-143.

Ovsiankina, M. Die Wiederaufnahme unterbrochener Handlungen. *Psychol. Forsch.*, 1928, 11, 302-379.

Pachauri, A. R. A study of gestalt problems in completed and interrupted tasks. *Brit. J. Psychol.*, 1935, 25, 447-457.

Pan, S. The influence of context upon learning and recall. *J. exp. Psychol.*, 1926, 9, 468-491.

Pascal, G. R. The effect of relaxation upon recall. *Amer. J. Psychol.*, 1949, 62, 32-47.

Perkins, F. T. Symmetry in visual recall. *Amer. J. Psychol.*, 1932, 44, 473-490.

Perry, H. M. The relative efficiency of actual and "imaginary" practice in five selected tasks. *Arch. Psychol., N. Y.*, 1939, 34, No. 243.

Pessin, J. The effect of similar and dissimilar conditions upon learning and relearning. *J. exp. Psychol.*, 1932, 15, 427-435.

Peters, H. N. The judgmental theory of pleasantness and unpleasantness. *Psychol. Rev.*, 1935, 42, 354-386.

Peters, H. N. Experimental studies of the judgmental theory of feeling. I. Learning of positive and negative reactions as a determinant of affective judgments. *J. exp. Psychol.*, 1938, 23, 1-25.

Peters, H. N. Experimental studies of the judgmental theory of feeling. IV. Retention of the effects of learned reactions on affective judgments. *J. exp. Psychol.*, 1939, 24, 111-134.

Philippe, J. Sur les transformations de nos images mentales. *Rev. Phil., Paris,* 1897, 43, 481-493.

Postman, L., and Page, R. Retroactive inhibition and psychophysical judgment. *Amer. J. Psychol.*, 1947, 60, 367-377.

Postman, L., and Postman, D. L. Change in set as a determinant of retroactive inhibition. *Amer. J. Psychol.*, 1948, 61, 236-242.

Pratt, C. C. The time-error in psychophysical judgments. *Amer. J. Psychol.*, 1933, 45, 292-297.

Pratt, C. C. The law of disuse. *Psychol. Rev.*, 1936, 43, 83-93.

Prentice, W. C. H. The interruption of tasks. *Psychol. Rev*, 1944, 51, 329-340.

Radossawljevitch, P. R. *Das Behalten und Vergessen bei Kindern und Erwachsenen nach experimentellen Untersuchungen.* Leipzig: Nemnich, 1907.

Raffel, G. The effect of recall on forgetting. *J. exp. Psychol.*, 1934, 17, 828-838.

Ratliff, M. M. The varying function of affectively toned olfactory, visual and auditory cues in recall. *Amer. J. Psychol.*, 1938, 51, 695-701.

Reed, H. J. The influence of a change of conditions upon the amount recalled. *J. exp. Psychol.*, 1931, 14, 632-649.

Rethlingshafer, D. Measures of tendency-to-continue. I. Behavior of feebleminded and normal subjects following the interruption of activities. *J. genet. Psychol.*, 1941a, 59, 109-124.

Rethlingshafer, D. Measures of tendency-to-continue. II. Comparison of feeble-minded and normal subjects when interrupted under different conditions. *J. genet Psychol.*, 1941b, 59, 125-138.

Rethlingshafer, D. Comparison of normal and feebleminded children with college adults in their tendency-to-continue interrupted activities. *J. comp. Psychol.*, 1941c, 32, 205-216.

Rickers-Ovsiankina, M. Studies on the personality structure of schizophrenic individuals. II. Reaction to interrupted tasks. *J. gen. Psychol.*, 1937, 16, 179-196.

Robinson, E. S. Some factors determining the degree of retroactive inhibition. *Psychol. Monogr.*, 1920, 28, No. 128.

Robinson, E. S. The "similarity" factor in retroaction. *Amer. J. Psychol.*, 1927, 39, 297-312.

Robinson, E. S., and Darrow, C. W. Effect of length of list upon memory for numbers. *Amer. J. Psychol.*, 1924, 35, 235-243.

Robinson, E. S., and Heron, W. T. Results of variations in length of memorized material. *J. exp. Psychol.*, 1922, 5, 428-448.

Rockway, M. R. The retention of paired-adjectives as a function of variations of a warm-up task. M. A. Thesis, Northwestern Univ., 1951.

Rosenzweig, S. Preferences in the repetition of successful and unsuccessful activities as a function of age and personality. *J. genet. Psychol.*, 1933, 42, 423-440.

Rosenzweig, S. Need-persistive and ego-defensive reactions to frustration as demonstrated by an experiment on repression. *Psychol. Rev.*, 1941, 48, 347-349.

Rosenzweig, S. An experimental study of "repression" with special reference to need-persistive and ego-defensive reactions to frustration. *J. exp. Psychol.*, 1943, 32, 64-74.

Rosenzweig, S., and Mason, G. A. An experimental study of memory in relation to the theory of repression. *Brit. J. Psychol.*, 1934, 24, 247-265.

Rohrer, J. H. Factors influencing the occurrence of reminiscence: attempted formal rehearsal during the interpolated period. *J. exp. Psychol.*, 1949, 39, 484-491.

Russell, R. W. Effects of electroshock convulsions on learning and retention in rats as functions of difficulty of the task. *J. comp. physiol. Psychol.*, 1949, 42, 137-142.

Russell, R. W., and Hunter, W. S. The effects of inactivity produced by sodium amytal on the retention of the maze habit in the albino rat. *J. exp. Psychol.*, 1937, 20, 426-436.

Sackett, R. S. The influence of symbolic rehearsal upon the retention of a maze habit. *J. gen. Psychol.*, 1934, 10, 376-397.

Sackett, R. S. The relationship between amount of symbolic rehearsal and retention of a maze habit. *J. gen. Psychol.*, 1935, 13, 113-128.

Sand, M. C. The effect of length of list upon retroactive inhibition when degree of learning is controlled. *Arch. Psychol., N. Y.*, 1939, 33, No. 238.

Sears, R. R. Experimental analysis of psychoanalytic phenomena. *In* Hunt, J. McV. (Ed.), *Personality and the Behavior Disorders*, New York: Ronald Press, 1944. Vol. I.

Sears, R. R., and Hovland, C. I. Experiments on motor conflict. II. Determination of mode of resolution by comparative strengths of conflicting responses. *J. exp. Psychol.*, 1941, 28, 280-286.

Sharp, A. A. An experimental test of Freud's doctrine of the relation of hedonic tone to memory revival. *J. exp. Psychol.*, 1938, 22, 395-418.

Siipola, E. M., and Israel, H. E. Habit-interference as dependent upon stage of training. *Amer. J. Psychol.*, 1933, 45, 205-227.

Sisson, E. D. Retroactive inhibition: the influence of degree of associative value of original and interpolated lists. *J. exp. Psychol.*, 1938, 22, 573-580.

Skaggs, E. B. Further studies in retroactive inhibition. *Psychol. Monogr.*, 1925, 34, No. 161.

Sones, A. M., and Stroud, J. B. Review, with special reference to temporal position. *J. educ. Psychol.*, 1940, 31, 665-676.

Spitzer, H. F. Studies in retention. *J. educ. Psychol.*, 1939, 30, 641-656

Stalnaker, J. M. Recognition and recall in a vocabulary test. *J. genet. Psychol.*, 1935, 46, 463-464.

Stalnaker, J. M., and Riddle, E. E. The effect of hypnosis on long-delayed recall. *J. gen. Psychol.*, 1932, 6, 429-439.

Strong, E. K., Jr. Two factors which influence economical learning. *J. Phil. Psychol. sci. Meth.*, 1914, 11, 124-131.

Stratton, G. M. Retroactive hypermnesia and other emotional effects on memory. *Psychol. Rev.*, 1919, 26, 474-486.

Stroud, J. B. Experiments on learning in school situations. *Psychol. Bull.*, 1940, 37, 777-807.

Swenson, E. J. *Retroactive inhibition: a review of the literature.* Minneapolis: Univ. Minnesota Press, 1941.

Swift, E. J. Memory of skillful movements. *Psychol. Bull.*, 1906, 3, 185-187.

Thune, L. E. The effect of different types of preliminary activities on subsequent learning of paired-associate material. *J. exp. Psychol.*, 1950a, 40, 423-438.

Thune, L. E. Warm-up effect as a function of level of practice in verbal learning. *Amer. Psychologist*, 1950b, 5, 251. (Abstract.)

Thune, L. E., and Underwood, B. J. Retroactive inhibition as a function of degree of interpolated learning. *J. exp. Psychol.*, 1943, 32, 185-200.

Titchener, E. B. Relearning after forty-six years. *Amer. J. Psychol.*, 1923, 34, 468-469.

Towne, B. M. An individual curve of learning: a study in typewriting. *J. exp. Psychol.*, 1922, 5, 79-92.

Townsend, J. C., Russell, R. W., and Patton, R. A. Effects of electro-shock convulsions on retention in rats as functions of intensity of electro-shock stimulus. *J. comp. physiol. Psychol.*, 1949, 42, 148-155.

Tresselt, M. E., and Spragg, S. D. S. Changes occurring in the serial reproduction of verbally perceived materials. *J. genet. Psychol.*, 1941, 58, 255-264.

Tsai, C. A comparative study of retention curves for motor habits. *Comp. Psychol. Monogr.*, 1924, 2, No. 11.

Tsai, L-S. The relation of retention to the distribution of relearning. *J. exp. Psychol.*, 1927, 10, 30-39.

Twining, P. E. The relative importance of intervening activity and lapse of time in the production of forgetting. *J. exp. Psychol.*, 1940, 26, 483-501.

Underwood, B. J. Associative inhibition in the learning of successive paired-associate lists. *J. exp. Psychol.*, 1944, 34, 127-135.

Underwood, B. J. The effect of successive interpolations on retroactive and proactive inhibition. *Psychol. Monogr.*, 1945, 59, No. 273.

Underwood, B. J. Retroactive and proactive inhibition after five and forty-eight hours. *J. exp. Psychol.*, 1948a, 38, 29-38.

Underwood, B. J. "Spontaneous recovery" of verbal associations. *J. exp. Psychol.*, 1949b, 38, 429-439.

Underwood, B. J. Proactive inhibition as a function of time and degree of prior learning. *J. exp. Psychol.*, 1949a, 39, 24-34.

Underwood, B. J. *Experimental psychology: an introduction.* New York: Appleton-Century-Crofts, 1949b.

Van Dusen, F., and Schlosberg, H. Further study of the retention of verbal and motor skills. *J. exp. Psychol.*, 1948, 38, 526-534.

Van Ormer, E. B. Retention after intervals of sleep and of waking. *Arch. Psychol., N. Y.*, 1932, 21, No. 137.

Van Ormer, E. B. Sleep and retention. *Psychol. Bull.*, 1933, 30, 415-439.

Ward, L. B. Reminiscence and rote learning. *Psychol. Monogr.*, 1937, 49, No. 220.

Waters, R. H. A case of environmental change with an accompanying loss of language. *Amer. J. Psychol.*, 1934, 46, 336-337.

Waters, R. H. Degree of learning and proactive inhibition in retention. *Psychol. Bull.*, 1942, 39, 495-496. (Abstract.)

Waters, R. H., and Peel, Z. E. Similarity in the form of original and interpolated, learning and retroactive inhibition. *Amer. J. Psychol.*, 1935, 47, 477-481.

Waters, R. H. and Poole, G. B. The relative retention values of stylus and mental maze habits. *J. exp. Psychol.*, 1933, 16, 429-434.

Watson, B. The similarity factor in transfer and inhibition. *J. educ. Psychol.*, 1938, 29, 145-157.

Wees, W. R., and Line, W. The influence of the form of a presentation upon reproduction: the principle of determination. *Brit. J. Psychol.*, 1937, 28, 167-189.

Wendt, G. R. An interpretation of inhibition of conditioned reflexes as competition between reaction systems. *Psychol. Rev.*, 1936, 43, 258-281.

Whipple, G. M. *Manual of mental and physical tests. Pt. II. Complex processes.* Baltimore: Warwick and York, 1921.

White, R. W., Fox, G. F., and Harris, W. W. Hypnotic hypermnesia for recently learned material. *J. abnorm. soc. Psychol.*, 1940, 35, 88-103.

Whitely, P. L. The dependence of learning and recall upon prior intellectual activities. *J. exp. Psychol.*, 1927, 10, 489-508.

Whitely, P. L., and Blankenship, A. B. The influence of certain conditions prior to learning upon subsequent recall. *J. exp. Psychol.*, 1936, 19, 496-504.

Whitely, P. L., and McGeoch, J. A. The curve of retention for poetry. *J. educ. Psychol.*, 1928, 19, 471-479.

Wickens, D. D. Studies of response generalization in conditioning. I. Stimulus generalization during response generalization. *J. exp. Psychol.*, 1943a, 33, 221-227.

Wickens, D. D. Studies of response generalization in conditioning. II. The comparative strength of the transferred and non-transferred responses. *J. exp. Psychol.*, 1943b, 33, 330-332.

Wickens, D. D. Studies of response generalization in conditioning. III. Stimulus variation as a factor in response generalization. *J. Elisha Mitchell Sci. Soc.*, 1945, 61, 258-263.

Wickens, D. D. Stimulus identity as related to response specificity and response generalization. *J. exp. Psychol.*, 1948, 38, 389-394.

Williams, O. A study of the phenomenon of reminiscence. *J. exp. Psychol.*, 1926, 9, 368-387.

Withey, S., Buxton, C. E., and Elkin, A. Control of rest interval activities in experiments on reminiscence in serial verbal learning. *J. exp. Psychol.*, 1949, 39, 173-176.

Wohlgemuth, A. The influence of feeling on memory. *Brit. J. Psychol.*, 1923, 13, 405-416.

Woodworth, R. S. *Experimental psychology.* New York: Holt, 1938.

Worcester, D. A. Retention after long periods. *J. educ. Psychol.*, 1923, 14, 113-114.

Wulf, F. Über die Veränderung von Vorstellungen (Gedächtnis und Gestalt). *Psychol. Forsch.*, 1922, 1, 333, 373.

Yum, K. S. An experimental test of the law of assimilation. *J. exp. Psychol.*, 1931, 14, 68-82.

Zangwill, O. L. The problem of retroactive inhibition in relation to recognition. *Brit. J. Psychol.*, 1938, 28, 229-247.

Zeigarnik, B. Das Behalten erledigter und unerledigter Handlungen. *Psychol. Forsch.*, 1927, 9, 1-85.

Zubin, J. The effect of electric convulsive therapy on memory. *Psychol. Bull.*, 1941, 38, 577. (Abstract.)

XI

LEARNING AS A FUNCTION OF THE MATERIAL LEARNED AND OF CERTAIN MODES OF PRACTICE AND PRESENTATION

THE observation has often been made in everyday activities and in the laboratory that apparently equal amounts of different materials take varying amounts of time to learn. This observation leaves no doubt that rate of learning is a function of the character of the materials learned. We wish to go beyond this, however, and to know how much the rate varies with specified changes in particular dimensions of the material. In the case of many learning situations, comparisons have not been made of rates of learning as a function of the dimensions along which the materials are distributed. There has been enough successful work with verbal materials to imply that similar analyses could be made in other learning situations.

The inference that rate of learning other materials than verbal ones is a function of the materials rests upon experimental observations, although those observations have not been along systematically studied dimensions. The high-relief finger maze (Miles, 1928), for example, can be made in a pattern equivalent in form and length to that of a stylus maze. The consistent result has been that the finger maze is the easier to learn. In the patterns used by Nyswander (1929) it required from one-third to two-thirds as much effort, depending on the measure, as did the stylus maze, and Husband (1928) found a still larger difference in terms of trials. The conditions which underlie the difference between the two may be, as Husband suggests, that the subject's contact with the path is more direct when it is traced with finger than with stylus; also, the paths not being open with two separated sides, they are quickly perceived as single, and the subject cannot pass a turn, whether true

path or blind alley, in the finger maze without realizing it. These may in turn act to reduce the need for trial-and-error exploration.

It would be possible to cite other comparisons which are methodologically legitimate, but they would tell us little because they have not been made with reference to any single dimension. We shall turn, instead, to the more systematic work on verbal materials. Information about the acquisition of verbal responses is peculiarly important because so much of human life is lived at the level of symbolic function, with words serving as the major class of symbol.

THE RELATIVE DIFFICULTY OF DIFFERENT VERBAL MATERIALS

Meaningfulness

Verbal materials are distributed along a dimension of meaningfulness, upon which their relative positions may be determined by methods of scaling or by simple ranking. Even the meaningfulness of nonsense syllables has been measured by Glaze (Chap. I) in terms of the percentage of subjects having associations aroused by each syllable. We can study learning as a function of meaningfulness by comparing the rates at which homogeneous groups of subjects learn comparable sections of material chosen from different points on the dimension of meaning.

There is general agreement that familiar three-letter words are more meaningful than even the most meaningful syllable which does not make a word. Nonsense syllables, which are also three-letter items, vary among themselves in meaning, as measured by associative value, and one method of studying the relation between meaningfulness and rate of learning is to compare the numbers of words and of syllables at different points on the scale of associative value which can be learned in a constant time. When 10-item lists of 3-letter words and of 0 per cent, 53 per cent, and 100 per cent syllables are studied with complete presentation for two minutes and then recalled immediately, the number recalled varies directly with the associative value (Table XXVIII). Though the words have the same physical constitution as the syllables in the number of letters, they are learned more readily than the 100 per cent syllables

and with less variability. Among the syllables, the number recalled varies directly with associative value. The table shows, then, a direct positive relation between relative meaningfulness and amount learned in a constant time.

TABLE XXVIII

THE AMOUNT OF DIFFERENT MATERIALS LEARNED UNDER CONSTANT EXTERNAL CONDITIONS

(From McGeoch, *J. genet. Psychol.*, 1930, 37, p. 425)

Material	Mean	$\sigma_{\text{Dis.}}$
3-letter words	9.11	1.12
100% syllables	7.35	1.96
53% syllables	6.41	2.37
00% syllables	5.09	2.60

Lyon's (1914) data upon the time required to learn equal numbers of items (syllables, digits, or words) show that syllables and digits are not far apart in difficulty, but that each is much more difficult than either prose or poetry (Table XXIX). Two hundred

TABLE XXIX

TIME REQUIRED TO LEARN EQUAL AMOUNTS OF DIFFERENT MATERIALS

(Data from Lyon, *J. educ. Psychol.*, 1914, 5. Once-per-day method)

Number of Items	Kind of Material	Time to Learn (Min.)
200	Nonsense syllables	93
200	Digits	85
200	Words of prose	24
200	Words of poetry	10

words of poetry were learned 9 times more rapidly than were 200 nonsense syllables. The prose and poetry were clearly more meaningful than the digits and syllables. No independent measure of meaningfulness has been applied to digits versus syllables or to

prose versus poetry, so that we cannot be sure of their exact locations on this dimension, but we can be sure of their difference in rate of learning. The greater difficulty of digits is shown further in an experiment by Reed (1924b, 1938) where 30 digits required 5.52 minutes for mastery, while a 30-word stanza of poetry took but 1.72 minutes.

Reed also found that nine lines of prose from a simple narrative required 111.25 seconds to learn to a criterion, while the same number from Hume on *The Origin of Ideas* took 261.25 seconds. When two excerpts of the same material, each containing 67 ideas, were read to college students, the mean number of ideas they could recall immediately was 11.5 from Hume and 49 from the simple narrative. Similar variations in rate of learning with variations in the material appear in the memory span studies. The width of the span may be from 4 to 20 times as great for words in meaningful sequence as for disconnected words, and it may be as great for words as for single letters, although the number of letters in each word may be nearly or quite as large as the number of isolated letters which can be reproduced. The materials mentioned may reasonably be assumed to lie some distance apart on the continuum of meaning. *The Origin of Ideas* almost certainly meant less to the subjects than did the narrative, while disconnected words clearly mean less than words in meaningful sequence. Meanings or "ideas" are, moreover, notably easier to learn than are meaningful verbal sequences learned by rote (Jones and English, 1926).

It is probable, on the basis of the available data, that there is a very high positive correlation between meaning and rate of learning. Certainly, the correlation is very high over limited ranges of meaning. This correlation requires examination. Does it signify that meaning is itself something independent of learning? By virtue of what properties does meaning play so great a facilitative function? The answers seem to lie within the field of learning, not outside it. The meanings of words are learned events, and the conditions of their operation are those which determine the influence of one set of learned events upon another. The influence of meaning seems, thus, to be a special case of transfer. When one says that Material A

is more readily learned than Material B because Material A is the more meaningful, one implies that A receives more advantage from transfer effects. This, in turn, is tantamount to saying that the learner already knew more about A at the beginning, or possessed more effects of prior training which could be brought to bear on the practice of A.

The dimension of meaning may be further analyzable, and there may be other dimensions closely related to it. Since neither possibility has been examined systematically, certain additional and related results will be cited without speculation concerning the dimensions on which they are distributed. Key's (1926) immediate recall records for words with commonplace relations in the Kent-Rosanoff list are considerably higher than those for words with unique relations. Abstract words, onomatopes, and concrete words, given one reading and recalled immediately, are learned with increasing ease in the order given (Stoke, 1929). Subjects can recall a large number of items from a picture, an event, or a card of objects after a very brief exposure time, but after the same amount of study time relatively few words from a list or lines from a poem could be reproduced. In one experiment, 60 items were recalled in narrative form after a 30-second exposure of the Binet Object-Card (McGeoch and Whitely, 1926). The large immediate recalls of this and similar materials are probably a function both of meaning and of the fact that the character of the materials permits the subject to perceive a large amount of detail quickly.

Affective Characteristics

Things learned are also characterized for the learner, by the facts of pleasantness, unpleasantness, or indifference, and it is a question whether rate of learning is a function of these characteristics and of their amount. Research interest has been concentrated on the influence of these characteristics on retention after an interval, and a brief treatment of their influence on learning will suffice. The learning materials for experiments on the problem have been classified as, P, U, or I on the basis of judgments made either by subjects

who later learned lists of words constructed from those judged or by individuals whose sole function was to classify the words. It is preferable to have the judgments made by the subjects who are to learn the lists, inasmuch as the criteria of affectivity employed are then more likely to be the same for both judge and learner.

Any procedure which classifies learning materials by means of judgments of affective tone implies at the level of the operations a judgmental conception of affectivity, such as that stated by Carr (1925) and elaborated by Peters (1935). Experimenters using the procedure may not have subscribed to the theory, but their experimental operations are those required by it. According to the theory and these operations, affection is a meaning. The theory states, further, that the meaning P is a function of a normal reaction tendency to approach the object, the meaning U of a reaction tendency to avoid it, and the meaning I of no particular tendency with respect to it. About this aspect of the theory the operations of the experiments in question imply nothing, but their use of judgments in the classifying of words as P, U, and I places the characteristics rated squarely within the category of meaning. As such, the results belong with those on meaning.

Although the results have not always been in agreement, words rated as P have frequently been found to be learned most readily, with U and I words following in that order.[1] This is consonant with the prediction to which the judgmental theory of feeling leads. Words judged to be pleasant on the basis of a normal positive response to them or what they represent should be responded to more actively, should be more likely to be learned by the devices of the logical method, and should probably have a richer meaning than words which are unpleasant or indifferent. Words judged to be unpleasant should be learned less readily than words judged to be pleasant, but considerably more readily than those regarded as indifferent and which normally elicit no characteristic response.

This frequent agreement between experimental results on learn-

[1] Representative evidence on the problem may be found in Cason (1932), Stagner (1933), Carter, *et al.* (1934, 1936), and Sharp (1938). Balken (1933) reports no difference in the rates at which P, U, and I words are learned.

ing as a function of affective judgment and the judgmental theory of feeling is not, of course, a crucial support of the theory. Feeling may be explained in other ways. The significant thing here is that the experiments have operationally treated feeling as a judgment and that the results are consonant with the theory. Other experiments have correlated rates of learning words in different affective categories with galvanometric deflections to these words and have sought in other ways for relations between rates of learning and measures of organic change. Many of the coefficients have been positive, some of them relatively high, but it is a question whether they should be interpreted as bearing on the relation here under discussion.

The Variable of Size

In the setting of this problem, size means the spatial dimensions of the letters in which verbal materials are printed or the dimensions of advertisements. The data on the problem are unanimous in showing that varying absolute size of type from one list to another is ineffective as a determiner of rate of learning, provided only that the materials to be perceived are clearly above the subject's lower threshold. Relative size, on the other hand, defined in the better-controlled experiments as variable size within a single list (different items printed in different sizes of type), or in the experiments upon advertising as variable size among comparable advertisements, sometimes has a positive influence and sometimes has none. The influence here is upon the learning of individual items within a series, where the larger items are, if anything, the more rapidly learned. When relative size does influence rate of learning, it seems to do so by virtue of novelty and "attention value" and by serving to introduce pattern and isolation into what would otherwise be monotonous materials.[2]

[2] A paper by Karwoski (1931) reports an experiment on absolute and relative size in lists of digits. The experiments of H. A. Peterson (1909) and Achilles (1920) bear on the problem. The papers by Adams and Dandison (1927) and Newhall and Heim (1929) give data on the influence of size of advertisements.

The Variable of Color

It may be asked whether color, as an attribute of other materials, affects the rate at which they are learned. The net result of the experiments on the problem is that the color in which learning materials are printed is unimportant, unless the material is of such a character that color acts as a differentiating factor. Van Buskirk (1932) has shown, however, that the introduction of a colored syllable in the least advantageous position within a list increases the rate of learning at that position. The best available interpretation of the role of color in learning is that it is an effective variable only when it acts to direct the subject's activity to the colored material.[3] The effects of color (and also of size) may be, therefore, an example of the Kohler-Von Restorff phenomenon. Introduction into a series of an item of unique color, shape, or size may be a method of producing "isolation" of that item.

MATERIAL AND CERTAIN MODES OF PRACTICE

Rate of learning is a function both of the material to be learned and of the way in which the subject attacks it. The interrelations and influence of these two variables is readily seen in studies of rhythm as a condition of memorizing and of logical or substance memorizing versus rote memorizing. In both cases, much depends on the subject. A person may respond to a highly rhythmical poem with a minimum of rhythm, treating it almost as if it were prose, or he may take full advantage of its rhythmic possibilities. His response depends not only upon the character of the material but upon his prior training and his present set.

The same is true of logical versus rote learning. In conventional usage, logical learning means the acquisition of meanings without any detailed mastery of the verbal sequences in which they are presented, while rote learning is the acquisition of a verbal series without any detailed concern for the meanings of the items. The

[3] For representative papers on the variable of color, cf. Achilles (1920), Brandt (1925), Pialat (1929), and Van Buskirk, (1932).

two are not all-or-none dichotomies, but are the extremes of a continuum along which methods of attack are distributed. The distinction between them depends both upon the material and the subject. A poem that is very meaningful to one subject may be almost meaningless to another, and even 0 per cent nonsense syllables may be woven into a meaningful pattern by one learner and regarded as so many unpatterned and refractory items by another. Other things being equal, however, the poem will probably be learned logically while the 0 per cent nonsense syllables are more likely to be acquired by rote.

The Influence of Rhythm

The early experimenters noticed that subjects introduced rhythm into their repetitions of nonsense syllable lists and that attempts to suppress the rhythm decreased the speed of learning (Müller and Schumann, 1894). Since then it has frequently been observed that subjects do this, even when successive items are presented at irregular intervals in order to make rhythmization difficult (Smith, 1907). In the latter case the rhythm is imposed by the learner. The less readily the material lends itself to being rhythmized, the more slowly does learning proceed (Elkin, 1928). The conclusion that the use of some rhythm as opposed to none accelerates learning provides one of the reasons why poetry is more easily memorized than prose.

The most advantageous rhythm and accent varies with the material and the individual. Müller and Schumann found trochaic rhythm superior to iambic and accounted for this fact by the further one that the common accent is on the first syllable of German dissyllabic words. The optimal rhythm may vary with the length of the list, as Adams (1915) has shown, and rhythms may be found which are less effective than no particular rhythm.

Acts of skill are often regarded as being less well-adapted than verbal materials to rhythmization. The timing of golf shots, typing, and many other perceptual-motor acts lies close, however, to the category of rhythm. Harding (1933) has studied the behavior of

trained typists and has found that the more rhythmical group in the typing of test words was the faster. The attempt to superimpose a rhythm aided some learners and hindered others.

The effectiveness of rhythm probably results in part from the way in which it assists in organizing the material into units which are readily perceived together, in part from the accent it gives to certain words or serial positions, which makes them stand out as reference points, and in part from the fact that it assists an active attitude on the part of the learner.[4]

Logical Memorizing versus Rote

The experiments on this problem fall into two classes. (a) Items individually possessed of little meaning may be arranged in some not-too-obvious pattern and the learning of the patterned lists compared with the learning of the same materials when the items are in a random sequence. This has been done by Guilford (1927) under the name of the role of form in learning. Examples of materials having such "form" or pattern are series of numbers arranged in some kind of numerical progression, lists of syllables having an orderly sequence of initial letters, or series of words in which sentences are concealed. The pattern took precedence for the subject over the individual items, permitting them to be learned as a meaningful series rather than by rote, and more rapidly than series which lacked such a pattern.[5]

(b) Two similarly constructed sets of materials may be learned, but in one case the subjects are instructed either to look for meaningful relations and to try to organize the items into a "logical"

[4] A more detailed account of the early experiments on rhythmical grouping is given by Woodworth (1938).

[5] This positive effect of patterning is confirmed by Klemm and Olsson (1925), who report that prompting in terms of some mathematical relation, such as a sum, is much more effective in the learning of series of four-place numbers than is mere verbal prompting. It plays a major role in Katona's (1940) comparisons of rote memorizing versus "understanding" in the solution of "match tasks" requiring, for example, the reduction of four squares made with twelve matches to three squares by moving only three of the matches. Snygg's (1935) experiments with tasks regarded as being "mechanically equivalent" clearly reveal the marked influence of a general principle.

sequence or to learn the ideas regardless of sequence in the passage, while in the other they are given the task of learning them as a rote series. This type of experiment differs from the one first described in that here the subject must organize the material meaningfully without the aid of a pattern already "planted" in the material by the experimenter. The results are continuous with those of the first, the search for or construction of a meaningful sequence being definitely superior to rote memorizing (Balaban, 1910).

In an experiment by Cofer (1941), which compared the repetitions and time taken to learn prose passages of four different lengths both verbatim and for the constituent ideas, the learning of the ideas was much the more rapid, being roughly three times as fast with the longer passages. That this is true has long been recognized in the educational emphasis on the acquisition of meanings rather than on the rote learning of words.

A wide variety of devices may be employed in the attempt to organize disparate items. Sequences of nonsense syllables suggest a word and are learned by means of this mnemonic aid; digits compose a familiar telephone or license number or date; meaningful words are supplemented by other words to make a sentence in which the words to be learned are the key ones; items are given arbitrary spatial relations, either absolute or relative; and in many other ways the items are fitted into the subject's repertoire of response.[6] The usefulness of these mnemonic devices is shown by the fact that in one of Reed's (1918) experiments Latin-English vocabulary pairs with which associative aids had been used were learned in half the number of prompts required by unaided pairs. As learning proceeds, however, the mnemonic aids tend to disappear; the items have become directly connected and are recalled without the mediation of the auxiliary devices. It is almost certain that associative aids peculiar to the material being learned and to the person learning it are more effective than artificial mnemonic schemes into which the material is forced.

[6] The three monographs by G. E. Müller (1911, 1913, 1917) are a mine of information about associative aids.

The Superior Effectiveness of Meaningful Relations

Whether we define meaning as associative value or in some other way, the meaning of materials learned is a much more potent determiner of the rate of their learning than are such characteristics as size or color. When the meaning of a material is not easily available to a learner, he may accelerate his rate of learning by a search for meanings, by the imposition of rhythm and pattern, by new groupings of the items, by noting spatial relations, and by other devices whereby he may make the material more meaningful and thus assimilate it more readily into his already existent patterns of response. This use of the so-called "logical" method is commonly superior to the rote method, but this need not be universal. Subjects differ considerably in the extent to which they employ a logical method on material which permits its use (Carlson and Carr, 1940). The relative effectiveness of the two methods may be a function of the specific materials practiced and of the habits and other characteristics of the learner. If one is habituated to the rote method, the attempt to employ a logical method might retard learning, at least initially, and an attempt to apply the logical method to material little susceptible to it might be less effective than rote learning.

The conclusion that there is a high positive correlation between meaningfulness of material and rate of learning holds under a very wide range of conditions. The greater effectiveness of the logical than of the rote method seems to hold over almost as wide a range. The first conclusion implies the generality of positive transfer; the second suggests that learning is not a passive chaining of adjacent items, but requires instead an active, analytic mode of response.

Meaningful Relations in Perceptual-Motor Learning

The imposition of meaning is not confined to the learning of verbal materials. Many subjects formulate a perceptual-motor problem, such as a maze, in terms of meaningful relations appropriate

to the problem. They formulate the correct path in terms of a verbal map by which they guide their movements, they note spatial relations, and in varied ways construct a meaningful pattern. The specific meanings are different from those of verbal materials, but their basic character seems to be much the same. Those who use the more verbal and meaningful methods of attack learn a maze more rapidly than do those who employ a "motor" method, "following the lead of their hands," without employing a pattern of representative devices (Warden, 1924b; Husband, 1931). In learning a foot maze, subjects may localize and guide their movements through orientation with respect to landmarks (W. Brown, 1932). Likewise, in Barker's (1931) temporal maze, subjects may rely on landmarks and meaningful relations.

All these are not radically different from the logical method in verbal learning. In both cases the learner is seeking to organize meanings to the end of controlling overt response thereby. The pervasive presence of representative devices and meaningful relations in the learning of perceptual-motor acts emphasizes the ubiquity of meaning.

THE SENSE ORGANS STIMULATED

Material to be learned must necessarily be presented to one or more of the sense organs of the subject. It may be presented in the precise form in which it is to be acquired or in the more general form of a problem situation, the solution of which is to be discovered and fixated. In either case the response to the material or act to be learned is mediated by the receptors. Even in the case of highly abstract rational problems, where the perception of the problem is the brief starting point of a long series of symbolic events, the fact of presentation is still discernible; only in the case of rational problems indirectly set by prior ideational activity does the presentational aspect of learning vanish.

Learning materials are describable, therefore, as visual, auditory, kinaesthetic, and so on through the list of sensory modalities, and their combinations. It follows that the problem of the relation between sense organ stimulated and rate of learning is intimately

connected with that of learning as a function of the character of the material. The factor of the sense organ has been treated by experimenters as independent because it is so often possible to control which sense organs are stimulated. One can compare rate of learning under visual stimulation with rate under auditory, but one must remember in interpreting the results that modality of stimulation does not wholly determine the subject's apprehension of the material. He may straightaway translate material presented to one sense organ into terms of other modalities. If unaccustomed to auditory presentation, he may attempt to imagine the material visually or to speak it subvocally or he may make implicit movements of writing or drawing it. The receptor is the starting point of the practiced response, but is by no means its sole determiner.

In view of this, it is remarkable that so much has been written about the influence of the sense organs upon the rate of learning. The net results with verbal materials may be summarized very briefly. Some experiments have found visual presentation of verbal materials superior to auditory (Gates, 1916; Koch, 1930), while others have found auditory presentation to be more effective (Henmon, 1912). Use of both visual and auditory stimulation in combination has usually been found superior to the poorer of the two when they are used separately and to be superior in some cases to either one (Smedley, 1902; Henmon, 1912; Koch, 1930). When, however, kinaesthetic stimulation in the form of movements of articulation is added to visual stimulation, to auditory, or to the two combined, the most frequent result has been an increase in rate of learning (Cohn, 1897; Barlow, 1928). Although the evidence available is less extensive, it seems probable that hand movements and perhaps other relevant responses may often exert a facilitating influence. The addition of kinaesthetic to visual or auditory stimulation exerts a positive effect which is more uniform in appearance and usually larger in amount than that of combined auditory and visual stimulation. While the conclusions stated in this paragraph have been reached primarily by the use of disparate serial lists, there is evidence that they hold in some cases for connected meaningful materials.

The differences which appear between modes are usually small and irregular in direction. It is apparent that no one mode of stimulation is generally most advantageous. Which mode of presentation will yield the most rapid learning in a particular instance depends upon a host of other conditions of the learning process and upon the characteristics of the individual learner.

Conditions of Which the Influence of the Sense Organ Stimulated Is a Function

(a) Rate of learning under a given form of stimulation is increased by *practice* with that form of stimulation, and practice seems to be the most potent variable in determining the influence of a particular mode. A student accustomed to learning from lectures or the radio may learn faster from auditory than from visual presentation by virtue of this practice, and, conversely, accustomedness to learning by reading may make visual presentation dominant. (b) *Chronological age* is a variable necessarily correlated with practice, since increasing age brings varying amounts of practice with the different modes of stimulation. The data indicate that auditory presentation is often more effective than visual for relatively young children and that it tends to become less effective as the age of the subject increases.

(c) *The material learned* constitutes a third variable. The learning of connected meaningful materials is often less affected by mode of presentation than is the learning of disparate items. This seems, in turn, to be a function of a fourth variable (d), *the mode of apprehension,* to which reference has already been made. The subtlety and richness of the representative and reactive devices of the human subject permit so ready a translation of the material into other terms than those in which it is presented that mode of stimulation may be unimportant, except in cases of very strong habituation to a particular mode. (e) *Individual differences* in ability to use different sensory cues are probably brought about in large part by the four variables here described. Carlson and Carr (1938) have shown that such individual differences exist and have suggested that they may

account for many of the experimental results on the influence of sensory modality.

Experimenters have suggested a number of inferential conditions upon which little or no quantitative evidence is available. The greater effectiveness of visual stimulation, when found, has been accounted for by the greater clarity of impression which it affords and of the unity with which it permits the material to be apprehended. Auditory presentation, on the other hand, in addition to its frequent lack of perceptual clarity, centers attention upon smaller units and gives to each a relatively brief stimulation. It seems, however, to arouse a more active response on the part of the learner. Indirect evidence in support of this observation is provided by the fact that in an experiment by Moore (1919) the immediate recall of factual material is considerably higher when the subjects hear it spoken without a manuscript than when they hear it read from a manuscript. Moore refers this difference to the greater attention value of the spoken material. This interpretation is supported by the fact that with another material, when the subjects under the two conditions were competing against each other, and when motivation and attention were approximately the same in the two groups, the spoken material was the better recalled by only a negligible amount.

When visual and auditory presentation are used together, subjects seem to be distracted by the lack of complete simultaneity of the two and lose whatever advantage would result from summation of the two stimulations, if other things were equal. The addition of subvocal articulation facilitates rhythmization, which in turn favors learning, and carries the advantage of reinforcement from the subject's common way of handling verbal materials.

Sensory stimulation is a necessary condition of learning, but learning occurs with much the same rapidity through any of the major modalities, granted only that practice with a given modality and a few other conditions are equal for each modality. The particular sense organ stimulated, or the sensory character of the material, whichever one chooses to call it, joins such characteristics as size and color as relatively unimportant determiners of rate of

learning. Meaningfulness stands out as the dominant condition, while these others become influential only when they increase meaningfulness, enhance motivation, or act in some other way than by virtue of their qualitative sensory or receptor characteristics only.[7]

The problem of the influence of the receptor stimulated is not confined to verbal materials, but is present wherever learning takes place. It figures in the influence of stimulating context, of muscular tension, of knowledge of results and effect, and of many other conditions to which we shall come, to say nothing of the question of the sensory control of habits in animals, which lies outside the scope of this book. Experiments on many of these conditions have demonstrated that stimulation of more than one receptor is somewhat more effective than stimulation through a single receptor.

DIFFERENT FORMS OF PRESENTATION TO THE SAME MODALITY

Different Exposure Methods

Material may also be presented in different ways to the same modality. These ways involve different spatiotemporal arrangements and, although a different arrangement may also alter the meaning of the material, the arrangement has been used as the objectively controlled variable. For example, Gordon (1903) compared the rates of learning when lists of syllables were exposed in five different ways: (a) successively along a horizontal line; (b) in regular succession around a circle; (c) successively at the same window; (d) successively, but at irregular points along the same line; and (e) in irregular succession around a circle. The first two methods of exposure gave the faster learning, but the differences involved are of the order of one trial. She interprets these and other results to mean that a coupling of regularity of exposure with a relative complexity facilitates learning. The complexity also introduced

[7] It should be observed that the relevant comparisons are between rates of learning material which is appropriate to the receptors concerned. There is no point to the question whether material which provides inadequate stimulation to a given receptor is as rapidly learned through it as through a receptor for which it is adequate.

novelty. The introduction of movement by drawing the materials on an exposure window or by drawing a blank card across the face of the card bearing the material to be learned slightly facilitates learning (Warden, 1926).

These are illustrative of spatiotemporal modes of presentation, the results upon which are continuous with those upon size, color, and sensory modality in showing very small influence upon learning. What influence spatiotemporal modes do have is probably exercised by means of the novelty they introduce, the greater activity they arouse on the part of the subject, and possibly the additional mnemonic cues they make available.

Temporal Sequence of Paired Associates

When we turn from the "mechanical" exposure methods to the variables which more directly involve the material and the subject's reaction to it, greater differences appear. A list of paired associates, analogous to a vocabulary list, may be presented in an unvarying order until it is learned, or in an order which changes the sequences of the pairs at each presentation of the list. The usual instruction to the subject is to learn the pairs without reference to order, to practice so as to be able to give the response term when the stimulus term appears, but in spite of this the order of the pairs makes a difference. When ten paired nonsense syllables and nouns are given five presentations, in one case with unvarying order and in the order with five different orders, the amounts recalled at the immediately following test are shown in Table XXX, in which N is 30 for each of the three lists practiced under each condition. The varying order yields a significantly smaller amount of learning at each sampling. Continuity of serial position, with the resultant connection of both adjacent and remote pairs or even of successive response terms only, which it makes possible, and the more stable set or expectancy which it engenders, yields much the faster learning. A varying order of presentation is commonly employed, however, because it forces the subject to associate the second member with the first without regard to serial associations.

Connections between the two members of a pair are more readily made when the more familiar or more impressive member occupies the initial position, the familiar members being common words and the unfamiliar ones nonsense syllables (Winzen, 1921; Cason, 1933), the more impressive one being a word such as *kiss, insane, vomit,* and the less impressive a word such as *paste, aim, neglect* (Thorndike, 1932). It may be by virtue of the greater associative context which it possesses and into which it assimilates the second member, and possibly by reason of the more effective set which it arouses, that the more familiar or more impressive member enters into association more readily when it comes first in the pair than when it comes second.

TABLE XXX

NUMBER OF ITEMS LEARNED UNDER SAME AND VARYING ORDERS OF PRESENTATION OF PAIRS

(From McGeoch and McKinney, *J. exp. Psychol.*, 1937, 20, p. 63)

	Condition and Sample					
	Same Order			Varying Order		
	1	2	3	1	2	3
Mean	7.60	8.43	8.70	5.46	5.86	5.90
σ_M	.45	.36	.29	.42	.48	.39

Perceptual-Motor Activities

Comparisons of the rates of learning perceptual-motor activities when they are presented in different ways to the same modality have been made chiefly with mazes, where the same pattern can be learned both as a stylus maze and as a high-relief finger maze. Traversal of both involves touch and kinaesthesis, but contact with the path of the stylus maze is through "a point of extended touch" (Miles, 1928), while in the finger maze the subject has direct cutaneous contact. The finger maze is learned the more rapidly of the two (Nyswander, 1929), probably because the more direct and complete contact with the path yields more complete information to the subject about the character of the path and the location of the blind alleys.

THE INFLUENCE OF AMOUNT OF MATERIAL

Whatever is to be learned must be practiced in some amount per practice period, which raises the question of the relation between the amount of material which is practiced as a unit and the time required to learn it. Does a list of 16 words take twice as long to learn as a list of 8, and one of 24 three times as long, or what is the relation? In this illustrative question the relation between increasing length of list and the times required to learn is one of simple linearity. Difficulty, as measured by time, might increase, however, at a disproportionately faster rate than the increases in length of list, or at a disproportionately slower one. The problem is to find the relation and the conditions with which it varies. The question of the influence of the amount of material will be recognized as another aspect of the total problem of learning as a function of the material and its modes of presentation.

Not all learning materials are composed of formally equal parts which lend themselves satisfactorily to arrangement in different amounts. Verbal materials obviously do; mazes with homogeneous alleys can be constructed with any desired number of them; some rational problems can be constructed in equal units; but ball-tossing, pursuitmeter practice, and many rational and relational problems have not been arranged thus and would be difficult to arrange.

Memory Span

A subject's memory span is the starting point of the present problem. The span is not a constant value, of course, but is a function of the age of the subject, the character of the items presented, the method of measurement, and other conditions.[8] The auditory digit

[8] Experimenters have used an unfortunately wide diversity of methods in measuring memory spans. Guilford and Dallenbach (1925) have collected 16 different ways of evaluating the number of items correctly reproduced, 9 systems of weighting the right responses, and 2 calculated scores which take into account the fact that difficulty is a function of serial position. Through these 27 methods runs only the common feature that they apply to immediate recalls of series presented but once. Add to them the diversity of conditions under which the items have been presented, and one has a picture of pointless

spans for college students show a fairly close grouping between 6 and 8.[9] Ebbinghaus' span for nonsense syllables was 7, and adults' spans for meaningful words are usually higher. They are, in turn, higher for sentences than for disparate words, thus corroborating the conclusion stated earlier in this chapter that there is a high positive correlation between meaningfulness and ease of learning.

The earlier investigators sometimes tacitly assumed that a span value was a constant, an all-or-none point beyond which a subject could not go. Later work has made clear, however, that a span value is an average and that there is a continuity of performance from points below this value to the much longer lists which require many more than one repetition for mastery (cf. Guilford and Dallenbach, 1925). Studies of the relations between amount of material and rate of learning have usually been made with lengths beyond the span, but a systematic view of the problem includes the span as the lower limiting case of the relation under one set of conditions, inasmuch as it is the longest list which can be entirely recalled after one repetition.

Span values are not lower limiting cases in the major experiments on the relation in question, because the operations of measurement employed with longer lists of material are not those used in determining spans. Spans are measured by starting with lists of items short enough to be recalled correctly by the subject and then extending the lists until the subject fails a specified number of times with a given length. Relations between amount of material and rate of learning are usually determined by starting with lists which are at or beyond the subject's span, and by methods, such as anticipation or paired associates, which do not readily permit determination of the span.

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diversity. In an experiment of their own, Guilford and Dallenbach have applied the principle of the psychophysical method of constant stimuli and the statistical limen to the determination of memory span for digits, defining the span as that length which has the probability 0.5 of being reproduced. The obtained spans vary between 6.7 and 9.9. Their paper contains a useful bibliography.

[9] The fact that the span values for college students group in the region from 6 to 8 is mirrored in the digit span required to pass at the adult levels of the Stanford-Binet Intelligence Scale (Terman, 1916; Terman and Merrill, 1937).

Amounts of Verbal Material Beyond Span Length

When learning is to some criterion, we are concerned with the relative amounts of work required to learn different amounts of the same material, using amount of work as a measure of difficulty. Trials required do not give us this information, because each trial is counted as one, regardless of the length of the material. Trials increase in temporal length as the number of items in the list increases. Thus, when presentation rate is constant, a list of 10 items takes only half as long to present as a list of 20 items. For the relation under discussion, then, time taken to learn is the basic measure.

Experimenters have seldom worked with such large amounts of materials as those studied by Lyon (1917), some of whose results are shown in Table XXXI. Time taken to learn both syllables and

TABLE XXXI

TIME TAKEN TO LEARN VARYING AMOUNTS OF MATERIAL

(From Lyon, *J. educ. Psychol.*, 1914, 5, and *Memory and the learning process*, 1917)

Syllables		Prose	
No. of Syllables	Time in Min.	No. of Words	Time in Min.
8	.13	50	2.25
12	1.50	100	9.00
16	3.67	200	24.00
24	5.00	600	84.00
32	6.00	1,000	165.00
48	14.00	3,000	780.00
72	25.00	5,000	1625.00
104	37.00	7,000	2065.00
200	93.00	10,000	4200.00
300	195.00	15,000	5475.00

prose passages increases at a rate which is disproportionately faster than the increase in amount of material. Where 50 words of prose could be learned in 2.25 minutes, 100 words required four times as long, and so on. The data do not show uniform relative increases in difficulty, but there is a clearly greater increase in time than is proportionate to the increase in number of items. This is shown by

the accelerating curve of Figure 52 in which the total number of syllables presented (which is the same as time taken, since presentation rate was nearly a constant) is plotted against the number in the list. When the data for prose are plotted, they show acceleration up to 10,000 words, at which point deceleration appears.[10] These

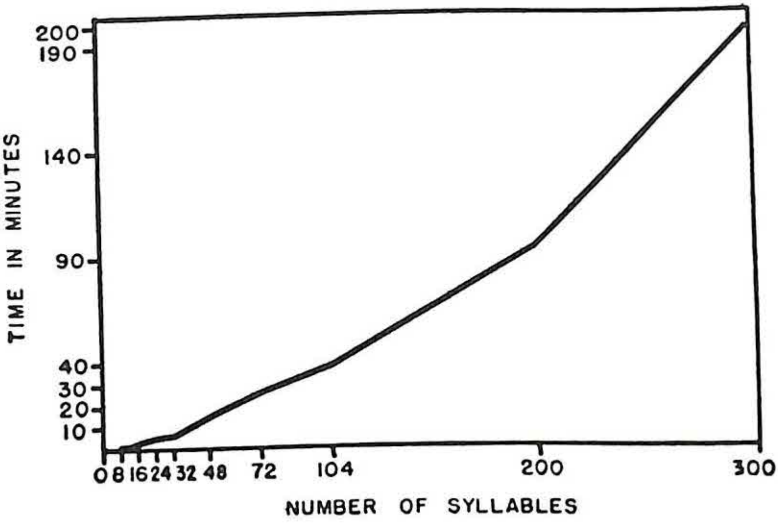

FIG. 52. A RELATION BETWEEN NUMBER OF SYLLABLES AND TIME TAKEN TO LEARN BY READINGS ONCE PER DAY

(From Lyon, *Memory and the learning process*, Plates I, VII, and VIIa)

The lengths actually used are recorded on the abscissa; the times actually taken are recorded to the nearest tenth on the ordinate.

data and the curve in Figure 53 for the relation between length of list and time required to learn it by the anticipation method are representative of a large amount of data. Whether the deceleration found by Lyon from 10,000 to 15,000 words of prose holds for very large amounts of other materials is unknown, but the disproportionate increase in difficulty (time) holds over the band of lengths used under ordinary conditions.

[10] Lyon's monograph (1917) contains a brief review of the work on this problem prior to 1917. While the conditions of Lyon's own experiment with himself as subject are not ideal, his data have been cited because they are the best available over a wide range of amounts and because they agree with those of other experiments over the lower region of this range and may be taken as representative.

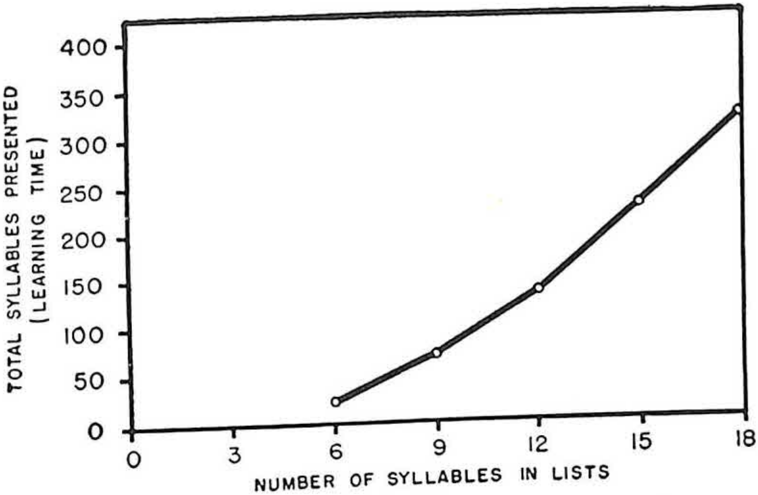

FIG. 53. A CURVE SHOWING THE RELATION BETWEEN RATE OF LEARNING AND LENGTH OF LIST

(From Robinson and Heron, *J. exp. Psychol.*, 1922, 5, p. 442)

Amounts of Nonverbal Materials

Some learning materials do not lend themselves unequivocally to use in experiments on this problem, even though they can be constructed in superficially equal units. Mazes with homogeneous alleys are composed of equal physical units, but under the usual conditions of practice the subject's response to them may be much more variable than his response to the units of a verbal series. During any given run from start to goal of a maze, he may enter any number of the alleys from very few to all of them, he may enter them in variable order (if retracing is allowed), he may spend amounts of time in each alley entered which differs from alley to alley, and he is free to determine his own rate of movement. The maze situation is very different from that of serial verbal learning with constant exposure rate, and measures of the relations between length of maze and difficulty are somewhat less clearly interpretable. Time and errors may be used here as measures of difficulty if one remembers the variability of response possible in a maze.

Results with mazes may be illustrated by the results of Scott and Henninger (1933), whose subjects learned the Miles (1931) X-maze

in lengths of 4, 5, 6, 8, and 10 alleys and the Warden (1924a) U-maze in lengths of 6, 10, 12, and 15, both in high-relief form. The curves for time (Fig. 54) and for errors (Fig. 55) approach in form the curve obtained by Robinson and Heron (Fig. 53), the only deviation being the time curve for the U-maze, which is linear. It is to be

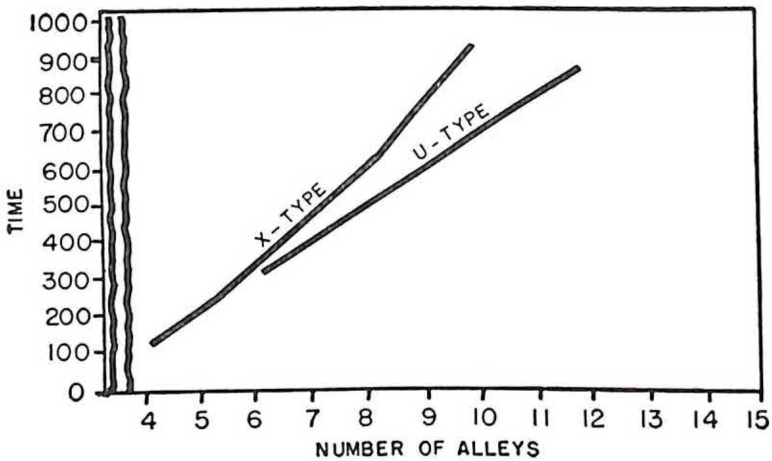

Fig. 54. Curves for Time in Learning Finger Mazes Plotted against Number of Blind Alleys
(From Scott and Henninger, *J. exp. Psychol.*, 1933, 16, p. 671)

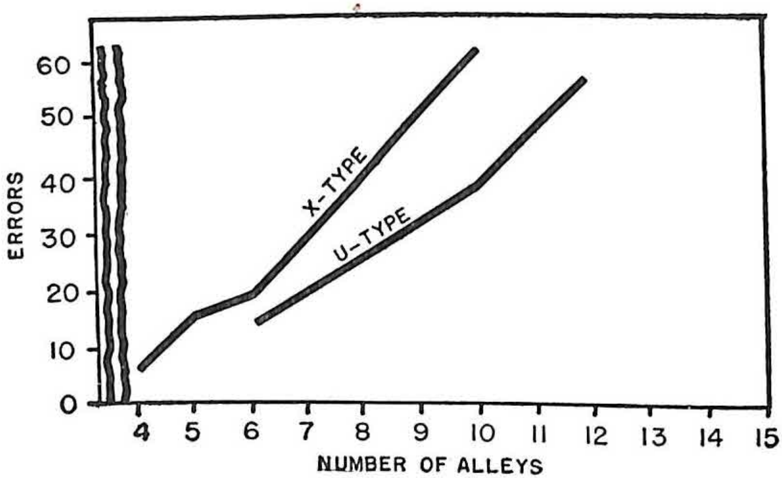

Fig. 55. Curves for Total Errors in Learning Finger Mazes Plotted against Number of Blind Alleys
(From Scott and Henninger, *J. exp. Psychol.*, 1933, 16, p. 670)

expected that the relations obtained with mazes should be a function of a large number of relatively specific variables, illustrated by the individual and shifting cues which may lead the subject to enter or to go past a cul-de-sac. Under some conditions, such as those of Scott and Henninger, however, results resembling those with verbal materials have been obtained.

Peterson Rational Learning Problems [11] (1918), which, in the pattern of activity required, lie between the acquisition of a fixed verbal series and the more selective process required in learning perceptual-motor activities, show an analogous disproportionate increase in difficulty with increasing length. In Table XXXII the

TABLE XXXII

RATIOS OF DIFFICULTY OF THREE PETERSON RATIONAL LEARNING PROBLEMS

(From McGeoch and Oberschelp, *J. gen. Psychol.*, 1930, 4, p. 157)

Problem	Pairs Presented	Time (Min.)	Logical Errors	Perseverative Errors	Unclassified Errors
6-letter	1.00	1.00	1.00	1.00	1.00
12-letter	5.55	7.17	12.51	82.28	8.01
18-letter	7.57(1.36)	10.57(1.47)	20.22(1.61)	137.04(1.66)	15.00(1.87)

scores made on the 6-letter problem are taken as unity in computing the major ratios, while the ratios between those on the 12-letter and 18-letter problems are given in parentheses. When the 6-letter problem is taken as the base, the two other lengths are more difficult to a degree which is markedly disproportionate to their lengths. The increase from 12 to 18 letters is accompanied by an increase in all measures, which is less disproportionate to the increase in length.

A corresponding disproportion has been found by Cook in mental

[11] The Peterson Rational Learning Problem consists of a series of letters, as $\underset{3}{\mathrm{A}}, \underset{1}{\mathrm{B}}, \underset{4}{\mathrm{C}}, \underset{2}{\mathrm{D}},$ with each of which is paired a number between specified limits, as between 1 and 4 in the short illustrative problem. The experimenter speaks the first letter, and the subject is to discover by trial and error which number is paired with that letter; then the experimenter calls out the next letter, and so on. A logical error is he guessing of a number already used for an earlier letter in the series; a perseverative error is repeating a wrong guess while reacting to a single letter; and unclassified errors are all other wrong guesses.

addition and subtraction (1937a) and in a disc transfer problem (1937b). His subjects solved problems in mental addition and subtraction involving 2, 3, 4, 5, and 6 digits, of which a sample 6-digit problem requiring both addition and subtraction was: $6 + 3 - 7 + 5 - 4 - 1$. They were instructed not to begin to solve the problem until the experimenter had finished reading the digits, so that the task was one in immediate memory as well as in simple arithmetic. The time increases much faster than the number of digits, roughly as the cube of the number of operations with 3, 4, 5, and 6 digits. The disc transfer problem, which is a form of puzzle requiring the moving of a set of blocks from one point to another in the least possible number of moves, was presented in lengths of 3, 7, and 15 moves. Again time and errors increase rapidly and disproportionately with length.

Conditions of Which the Influence of Amount of Material Is a Function

Over a considerable range of lengths and of materials, a disproportionate increase in difficulty, as measured by time taken to learn and sometimes by errors, has been found. Not all materials are amenable to construction in comparable units, however, and not all of those which might be arranged thus have been studied. As we have seen, the time curve for the U-maze employed by Scott and Henninger is linear, and it may be that the length-difficulty relations are functions of the material learned. There are other conditions, however, which have been more clearly shown to influence the relation.

(a) The *practice level* attained by the subject at the time the records are taken may influence the relation, the disproportionality between length and time taken to learn being less at higher levels of practice, but there is no certainty that even a high degree of practice can remove the fact of disproportionality, especially with disparate serial lists, although it may change the slope of the time-length curve.

(b) In Lyon's (1917) experiments the disproportionate increase

in time for learning increasing numbers of syllables and digits was more pronounced when practice was *massed* than when it was *distributed*, but the difference between massing and distribution is small when prose and poetry constitute the material. A similar influence from distribution appears in the time and error records of Cook's (1937d) experiment with finger mazes in 4, 8, and 12 units and in Hovland's (1940) data for nonsense syllables in lists of 8, 11, and 14.

(c) It makes a difference whether measurement is by *immediate recall* or by *learning to a criterion*. The subjects may be given but a single exposure of syllable lists of different lengths, followed by immediate recall, so that one influence of length is measured by the amount recalled at each length. Shurrager (1940) has done this with lists varying from 3 to 36 syllables and has applied the Thurstone-Woodrow (1925, 1936) scaling technique to the results. The mean per cents correct fall at a decelerated rate when plotted against length, and the scaled difficulty values rise with deceleration. This is analogous to the results obtained by Calhoon (1935). The failure to find the disproportionality between length and difficulty which so often appears when learning is to a criterion may be ascribed to the method of single presentation with immediate recall.

(d) Time taken to learn increases much more steeply with increasing length when prose passages are learned verbatim than when they are learned for the constituent ideas—that is, by the *logical* method (Cofer, 1941).[12] Material is here a constant, while instruction, set, and method of learning vary.

(e) Related experiments permit us to infer that still other con-

[12] The increases in trials with increasing length of passage, on which Cofer (1941) places the greater emphasis, are decelerated, as have been the trial scores in a number of experiments. The discussion in the text has been limited to measurements in terms of time, because time is the more unequivocal unit in which to measure the difficulty of materials of different length, where trials vary in temporal duration as length varies. In Cofer's experiment, which is of high importance as a study of verbatim versus logical learning of the same materials, a trial consists both of a single reading and the immediately following attempted recitation and is therefore of inconstant duration even with a single length of passage. For the present purpose, the time records are the more important.

ditions are important. Among these others are the *characteristics of the subjects*, such as age, intelligence, and species, the *magnitude of the groups with which the subject is able to work*, and the *instructions given to the subject.*[13]

HYPOTHESES ABOUT LENGTH-TIME RELATIONS

An adequate hypothesis should account for the fact that difficulty of learning increases with increasing amount of material and that it so often does this disproportionately. Relatively little effort has been directed at finding such an hypothesis. Certain of the suggestions of the early investigators are not now acceptable.

(1) Offner's (1909, 1924) supposition that, as material increases in amount, less attention is paid to each item seems to assume a constant amount of attention which must be attenuated if it is to spread over a long list, but which is relatively concentrated if the list is short. This view cuts across the working conceptions of contemporary research and would be, in any case, difficult to defend in the light of current controlled exposure methods and careful instructions.

(2) The fatigue theory of Myers (1925), which assumes longer series to be more fatiguing and hence to receive less attention, is related to Offner's. Certain difficulties with fatigue theories of psy-

[13] Several attempts have been made to state the relation between length of list and difficulty of learning in the form of an equation which is usually put forward as representing a general law. In connection with his rational equation for the learning function, Thurstone (1930) has formulated the hypothesis that the relation between number of items in a list and the time required to learn would follow approximately the following law:

$$T = \frac{c}{k}\, n\sqrt{n-a}$$

in which T is the total learning time, k is the subject's learning constant, c is a criterial constant, n is the number of items in the list, and a is the subject's attention (memory) span. The hypothesis, which is limited to meaningless material learned by a single practiced subject, has been tested on data from Lyon and others and has been found to yield a satisfactory fit. The Thurstone equation adequately describes the data gathered under the experimental conditions obtaining, but how far it can be generalized to other conditions is yet unknown. The fact that the phenomena of learning are functions of their determining conditions does not mean that results upon them cannot be described in equational terms, but it does mean that the generality of any equational expression must be tested upon experimental data before it can be known.

chological phenomena have been mentioned in Chapter V. Such a theory is contradicted here by the fact that shorter lists are easier than longer ones under distributed practice, even when the repetitions of each are at the rate of one per day and when fatigue must have been minimal, and by the further fact that the increase in difficulty is out of proportion to the increase in length. The lack of difference between the length-time relations under massed and distributed practice when meaningful material was learned, although a difference did appear with disparate items, strongly implies that some other conditions than fatigue is basic.

(3) Practice involves both work and learning, but in most experiments the latter masks whatever decrement may accompany the former. Bills and Brown (1929), in an experiment designed to investigate work rather than learning, have discovered a condition which seems applicable to the length-time problem and which includes most of whatever validity the two theories first mentioned may be alleged to possess. They find that the steepness of the work decrement varies directly as the amount of work with which the subject is faced at the start, or with his quantitative *amount set*. It is to be expected that a subject's amount set will correspond to the length of list he has before him for practice, and hence the longer the list, the greater will be the work decrement and the more will learning be retarded. This may be one effective condition, but it can scarcely be the major one. It is offered here as one possibility, although we do not know from direct test whether amount set operates in learning experiments as it did in the work experiments where it was discovered.

(4) Carr (1925) has suggested that the increase in amount of needless repetition of already learned items which occurs with increasing amount of material may account in part for the length-time relations found with verbal materials. Some parts are learned before others, and the ones learned earlier are unnecessarily repeated while the later ones are being acquired.[14]

[14] Continued repetition of items already learned can be avoided in experiments with paired associates by using Woodworth's (1914) method of dropping each pair from the list as soon as its response member has been recalled a specified number of times. This has been done by Sand (1939).

Both needless repetition of items already learned and the phenomenon of amount set may be conditions of obtained length-time relations, but it seems probable that a theory in terms of intraserial inhibition is more general. The two theories of this kind that were discussed in Chapter IV as theories of serial position phenomena also apply here.

(5) Hull's (1940) hypothesis that the bowed serial position curve is a function of the remote excitatory tendencies spanning intermediate items and that total amount of inhibition is given by the number of these tendencies which span any given item applies to the length-time relations also. As length of series increases, the number of spanning excitatory tendencies and the amount of inhibition, and therefore difficulty of learning, will increase. Thus, in lists of 7, 9, and 11 items, the numbers of possible forward excitatory tendencies spanning the middle items are 9, 16, and 25, respectively. If inhibition increases as these numbers do, and if difficulty increases as does inhibition, the obtained experimental results would follow.[15]

(6) The second theory in terms of intraserial inhibition ascribes the length-time relations to the combined operation of proactive and retroactive interference. The addition of each item to a series increases the number of items which may exercise both a proactive and a retroactive influence. It will be remembered that Foucault (1928) discovered both that the number of words in the first position which were forgotten and, to some extent, the number in the last position increased as the list lengthened and that the combined action of proactive and retroactive inhibition is greater than the simple sum of the inhibitory influences of the number of preceding and following words, measured in lists containing that number. This theory, also, can account for the result that difficulty increases faster than length.

Either of these interpretations in terms of intraserial inhibition is reasonable. At the present time the second appears the more able

[15] The statement in the text is an oversimplification of the Hull position. For a more detailed statement than can here be given, the student should consult the original monograph (1940), where a number of important theorems and corollaries about learning as a function of length of list are stated.

to incorporate the data upon other materials than serial lists. Whichever form of inhibition theory is supported by future research, there is the possibility that it will be supplemented in some degree by the action of other conditions, such as amount set and needless repetition of already learned items.

WAYS OF DIVIDING THE MATERIAL TO BE LEARNED: WHOLE *VERSUS* PART METHODS OF PRACTICE

A learning material or activity which can be divided into parts may be practiced in either of two different ways: (1) It may be repeated as a *whole*,[16] in the sense that it is repeated from beginning to end at each trial until the criterion is reached, or (2) it may be divided into two or more *parts*, each of these parts being practiced as a separate unit and then connected with each other. The second or *part method* divides into certain subclasses, some of which approach the whole method. Three of them will be described. (a) In the *pure part method* each part is separately learned to a criterion, after which all the parts are repeated in sequence as a whole until the whole has been brought to the criterion already attained by each part. (b) By the *progressive part method* Parts 1 and 2 are mastered separately and then combined to form a whole. Part 3 is next learned separately, then practiced with Parts 1 and 2, and so on by the progressive addition of parts until the original whole has been learned. (c) The *repetitive part method* consists of mastery of Part 1, then practice of Parts 1 and 2 together, next of Parts 1, 2, and 3 together, and so on until all have been learned.

Whether learning is more rapid by the whole method or by some variant of the part method is a question which has stimulated a large amount of experimentation, beginning with Steffens (1900) and continuing through Pechstein's (1917) and Reed's (1924a) provocative research to the more recent work of Crafts (1929, 1930,

[16] A majority of the experiments on this problem have employed a quantitative definition of whole and parts, such as that a poem of ten stanzas is the whole and each block of two stanzas is a part, or that a total maze is a whole and each of four sections is a part. Seagoe (1936a), however, has employed a gestaltish conception of a whole as a qualitative unit. In this connection, see also a paper by Rubin-Rabson (1940).

1932), Cook (1936; 1937c, d; 1939a, b), and others.[17] The conclusions drawn from some of the early work rested on small differences of unknown or doubtful significance and reflected a constancy attitude, according to which one method or the other was assumed to be generally the more effective. The later work has shown that the relative effectiveness of the whole and part methods is a function of a number of conditions, and this section will analyze the problem and describe the major conditions on which results have been found to depend.

The whole-part problem is continuous with that of the relations between length of material and time taken to learn, with the addition of the recombination of the separately learned parts into a single unit. Can a whole of a given amount be more rapidly learned when practiced as a whole or in parts which are later combined? The problem may be analyzed into three parts or subproblems: (a) First of all is the question of the time required to learn the individual parts. Since larger amounts of material require disproportionally longer times than shorter ones, it is to be expected that the sum of the times required to learn the parts will be less than the time required to learn them when they are practiced as a whole. It follows, also, that the greater the number of parts and hence the smaller each one is, the greater will be the difference between the sum of the time for learning the parts and the total time for learning them as a whole. (b) When practiced separately, however, the parts must finally be combined, and here a new factor is introduced beyond the features of the time-length relation. The relative effectiveness of whole and part methods will be a function of the time taken to connect the parts into a whole. This time may be sufficient to overweigh the saving from having practiced the material in smaller amounts, or it may be small enough to leave a significant balance in favor of learning by parts. (c) There are other conditions involved in practicing a number of short sections of material in sequence which might influence the time-length relations of the

[17] Extensive bibliography on the problem, together with a discussion of many of the papers, may be found in G. O. McGeoch (1931b) and Seagoe (1936a).

individual parts. Unless the subject is at a practice level to begin with, practice at successive parts will be accompanied by positive transfer, and he will learn successive parts at a faster rate. As he proceeds through the sequence of the parts, however, there will also be some negative transfer by virtue of the interrelations of the material. This forgetting of earlier learned parts will decrease the advantage gained from working with smaller amounts of material and will leave a relatively greater opportunity for the whole method to prove more effective, unless the parts can be connected quickly enough to overweigh this decreased advantage.

The results of the experiments on the problem have been far from uniform. Differences have been found in favor of the whole method, the pure part method, and the progressive part method, especially. While it is not yet possible to formulate the conditions which determine the result in a way to make accurate prediction possible, some of the important ones are known and will be described.

CONDITIONS OF WHICH RESULTS WITH WHOLE AND PART METHODS ARE A FUNCTION

The Characteristics of the Subjects

Under many conditions the amount and direction of the differences between the methods are a function of the intelligence of the subjects. The more intelligent the subject, the more likely is the difference to be on the side of the whole method and the larger is it likely to be (Pechstein, 1926; G. O. McGeoch, 1931a). There is evidence, also, that with increasing chronological age the whole method becomes relatively better than the part methods. Taken together, the data imply that the higher the level of mental development, the more likely is the whole method to be superior.

Practice with the Methods

Several of the early experiments had found that practice yielded an increasing difference in favor of the whole method. For example, subjects would find a part method superior at the beginning of prac-

tice but the whole method the more effective after practice. More recently, in a direct study of this condition, Wylie (1928) has shown that the advantage of the whole method increases with practice.

Distribution of Practice

Any of the methods under discussion may be used with massed practice or with any form of positive distribution. Pechstein (1917) has concluded that distribution favors the whole method of maze learning and massing of practice a part method, a conclusion which has been corroborated by Winch (1924) and Sawdon (1927) with verbal materials, and by Crafts (1930) with substitution.

The Material Learned

It has frequently been suggested that, as the meaningfulness and continuity of the material increases, the whole method should become increasingly more effective, because it permits the subject to grasp more readily the continuous pattern of the material. Conversely, the more disparate the material, the more should the advantage shift to the part methods. Some experimental results fulfil this expectation, but others do not. The considerable number of experiments which have found faster learning of connected and unified material by the whole method fulfil it. One by Seagoe (1936b), for example, in which a whole was qualitatively defined, employed four designs of differing degrees of perceptual unity, each divided into parts which were to be assembled according to the pattern. The two more integrated designs were more quickly learned in terms of time, trials, and errors by the whole method, while to some extent the less integrated ones were more rapidly learned by a part method.

That meaningfulness and continuity of material is not a crucial condition is demonstrated, however, by the fact that a part method is sometimes superior with poetry and other continuous material and the whole method is sometimes superior when serial connections and continuity are minimal. On the latter point, Crafts (1929,

1930, 1932) has found the whole method to be more effective in learning card-sorting, substitution, and visually presented circles and figures.

There is reason to believe, however, that meaning and unity are important conditions favoring the whole method, but that they may be outweighed by other conditions, when these are present in sufficient degree. Conversely, disparateness of materials and relative lack of meaning, which seem to favor the part methods, may be counterbalanced by other conditions which give advantage to the whole method.

The Lengths of the Whole and the Parts Compared

Other conditions being equal in effect, the facts of the time-length relation favor a part method of practice. Not only are other things seldom equal, however, but the relative sizes of the parts and the whole will make a difference in the advantage which accrues from the time-length relation. To illustrate this, let a list of 18 syllables require 700 seconds to learn and let the three 6-syllable parts of an equivalent list require 100 seconds each or 300 seconds altogether. If the remaining 400 seconds are not consumed in connecting the parts, the part method will be superior. But take another example in which the list is 54 items long and the parts 18 each. Assume that the time-length relation is such that the whole list requires 2200 seconds for mastery and each of the parts 700 seconds or a total of 2100 seconds. There is now a difference of 100 seconds, which may easily be taken up in connecting the parts, so that the two methods will show no difference. These hypothetical values illustrate the way in which the lengths of the parts and of the whole, the slope of the time-length function, and the positions of parts and whole on it may influence the outcome of an experiment. Comparisons of whole and part methods by different experimenters which yield different results in spite of use of closely similar materials may do so because the wholes employed, the parts, or both, are of different size.

The Time Required To Connect the Parts

The advantage which accrues to the part methods from the relatively greater ease of shorter than of longer materials is counterbalanced in direction by the fact that the parts, after having been learned individually, must then be connected. The proportion of the total learning time spent in connecting the parts will vary with conditions, but it is often large. Pechstein's (1917) human subjects, working under distributed practice, made 80 per cent of their total errors while connecting the four parts which had been learned separately. In three different experiments by Cook, Morrison, and Stacey (1935), 28 per cent, 29 per cent, and 29 per cent of the errors were made during the combination of the parts, while in one experiment by Hanawalt (1933) animals in two different mazes required 29 per cent and 42 per cent of the errors during combination of parts, in another roughly two-thirds of the total (1931), and in one with human subjects approximately two-thirds (1934). These data are for mazes, but although similar records are not available, it is probable that somewhat analogous difficulty is encountered in combining the parts of verbal material.

In view of the fact that the parts have already been learned to a criterion, the additional practice required to combine them calls for explanation. The most serious attempt at this has been made by Pechstein in terms of place associations (1917), which mean the association of any aspect of the problem with some other aspect which is specific to that particular problem. In maze learning, distance and specific turns may be associated with any other aspect of the pattern, especially with the goal, but the goal and the setting of many other connections change when a part becomes continuous with other parts to form the total maze. Likewise, in verbal material, especially by massed practice, the last item of a part may become associated with the first item of the same part, because in practice the subject has gone directly from the last item back to the first and started reading the material again. Such associations appear overtly in the behavior of the child who, when reciting a poem, goes from the end of a stanza to the beginning of that stanza or of one

earlier in the poem. Pechstein's observations on both human and infra-human subjects support his explanation in terms of place associations. While combining parts, subjects would stop at what had been the goal and go back to the beginning. Certainly, in combining parts learned by the part method a large proportion of the subject's effort is spent in connecting the parts, and place association is one reasonable condition thereof.

Why do place associations make difficult the combination of the parts? For one thing, they involve associative inhibition. When a subject has connected the end of Part 1 of an activity or material with going back to the beginning of that part, he will find it more difficult under many conditions to associate the end of that part with continuing on to the beginning of the next one. Likewise, when the subject shifts from practice by parts to practice of the whole, the relation of the individual responses to the goal is changed in all except the last part, the subject's set may be altered, and confusion may result.

Whatever the conditions which bring about the difficulty of connecting the parts, they are not impossible to overcome. The pure part method, progressive part method, and other modifications have sometimes yielded faster learning than the whole method. Place associations and the other conditions producing difficulty of part-connection doubtless vary in amount and influence. They have probably been less effective in the experiments which have shown a superiority for a part method than in those which have found the whole method better.

Positive and Negative Transfer

At the beginning of the discussion of whole and part methods mention was made of the fact that positive transfer or practice effects could occur from part to part, and that negative transfer or interference could occur from later parts to the recall of earlier ones at the time of combination.[18] Experimenters have observed the

[18] Pechstein (1917) concluded that in maze learning by the part methods positive transfer will operate to facilitate much more than it will by the whole method, but Barton (1921) found as much transfer by whole practice as by

presence of transfer, and it is almost certainly an important condition of results on the whole-part problem. From our knowledge of both positive and negative transfer as a function of the relations between materials it is to be expected that the influence of the character of the material used and of transfer need to be considered together. Insufficient quantitative data are available upon transfer in the setting of the present problem to permit a generalization.[19]

A Practical Summary

The whole-part problem is not one upon which an easy generalization can be made. One of its chief values, in the present state of knowledge, is the way in which it illustrates the complexities resulting when one set of phenomena, the time-length relations of separate parts, is combined with another, the time-length relations of those parts combined into a whole.

The part methods have the advantage that they subdivide the material into small and relatively easier units, with all that this implies. The whole method has the advantage that it obviates the time lost in connecting previously learned individual parts, gives the subject a greater opportunity to grasp the interrelations of the material, and distributes the responses to any given item. Especially if the subject is relatively mature and practiced with the whole

part, while Hanawalt (1931, 1934) failed to find positive transfer between parts. Transfer may also change from positive to negative, or the reverse, during practice (Beeby, 1930).

[19] Many other conditions have been suggested by the experiments on the whole-part problem, but they have not been mentioned in the text because their influence is too little studied to enable us to state the extent of their generality. Some of them are: (a) the units in which learning is measured; (b) the optimal size of group with which each subject can work; (c) the degree of familiarity and difficulty of the learning task; (d) the motivation of the subject and his amount set; and (e) the specific mode of division into parts. On this last point something further should be said. The usual experiment breaks an act or material into spatiotemporal parts which are to be learned separately and then combined sequentially. An experiment by Koch (1923) has dealt with unit responses which may be learned by either hand separately or by both hands together. The division here is in terms of the motor organs involved. The part method gave the faster learning. R. W. Brown (1933) has reported that the "hands-together" (whole) method of learning to play piano scores gave faster learning than the "hands-separate" (part) method, but the result may have been a function of prior practice.

method, it is likely to be the more effective. But in view of the number of conditions which determine the results and of the extent to which results are susceptible to these conditions, the chances are high that a subject may use whichever method he has practiced and prefers without fear that he is employing an extremely inefficient one. Practically, one might well follow Woodworth's (1938) suggestion and employ the whole method with special attention to and repetition of difficult or important parts as one goes along, thus combining the whole method with a form of the part method.

RECALL DURING PRACTICE

There are wide variations in the extent to which a subject depends on the direct stimulus pattern of the material being practiced. Assume that he is memorizing a list of words. At the extreme of complete dependence on the stimulus pattern he repeats each word as he perceives it, but does not attempt to anticipate any word or to recall a word before it is actually presented. At the opposite extreme, beginning with the second presentation he attempts to recall as much as possible of the material without having it presented to his receptors. Between the extremes lie innumerable combinations of presentation and independent recall.

The usual experimental procedure has been to present the material for a given time or number of presentations, and then to instruct the subject to recall as much of it as possible without the copy, prompting himself from the copy when recall fails. The conclusion for verbal materials has been that some degree of recall (or recitation) with prompting during practice is a more effective condition of learning than is presentation alone. The few experiments which have studied recall without prompting have obtained a similar result.

Conditions of Which the Influence of Recall Is a Function

(1) *Relative amount of time spent in recall.* An experiment by Gates (1917), which has yielded results decisively in favor of recitation with prompting as a facilitating condition of learning, used

lists of nonsense syllables and short biographies. Under the presentation condition, school children spent the entire practice time in reading, while under the recall conditions the last 20, 40, 60, or 80 per cent of the practice time was spent in recitation plus prompting. The subjects were instructed not to attempt to recall during the portions of the time devoted to reading alone.

The results in terms of immediate recall of nonsense syllables (Table XXXIII) show for each grade a uniform increase in amount learned as the relative amount of time spent in recall increases from

TABLE XXXIII

MEAN SCORES (IMMEDIATE RECALL) IN LEARNING NONSENSE SYLLABLES WHEN VARYING PERCENTAGES OF THE TIME ARE SPENT IN RECITATION *

(From Gates, *Arch. Psychol.*, N. Y., 1917, 6, p. 36)

Subjects		Percentages of Time Spent in Reading (P) and in Recitation (R)				
		P100-R0	P80-R20	P60-R40	P40-R60	P20-R80
Grade 8	M	16.92	23.86	25.79	27.28	35.51
	P.E.	0.61	0.69	0.65	0.66	0.86
Grade 6	M	13.21	20.18	22.64	25.15	30.52
	P.E.	0.61	0.84	0.60	0.91	1.07
Grade 4	M	9.45	12.00	16.10	16.95	20.05
	P.E.	0.57	0.46	0.56	0.75	0.79

* Each grade contained from 40 to 45 pupils. Grade 8 was given 15 syllables, Grade 6, 15, and Grade 4, 14. The total practice time was 9 minutes.

zero to 80 per cent. When 80 per cent of the time is spent in recitation, the scores are more than twice as large as those when all of the time is devoted to reading alone.[20] Recitation, though still superior to reading alone, is less effective with short biographies than with syllables, but it is important that recitation for as much as 90 per cent of the time yields higher scores than reading alone.

(2) *Temporal point of introduction.* Varying numbers of read-

[20] Forlano (1936) has employed Gates' major conditions under schoolroom conditions with results which demonstrate with spelling, nonsense syllables, and hard English vocabulary a greater effectiveness with increases in the proportion of the total time spent in recall are more irregular than those of Gates.

ings may be permitted before recitation begins, especially when practice continues until a criterion is reached. Skaggs and Grossman (1930) have compared the introduction of recall plus prompting after 1, 3, 5, and 7 initial presentations of lists of 8 nonsense syllables learned by a modified anticipation method. The number of trials with prompting required to complete the learning to one perfect repetition decreases as the number of initial presentations increases, but if we add together the presentations and the prompted trials, learning is most rapid when only one presentation is given before prompting begins. Learning is most rapid in terms of time when three initial readings are given before recitation begins. Results on the problem agree that early introduction of recall with prompting is optimal.

Introduction of a single recall *without prompting* increases the learning score on lists of 20 pairs of monosyllabic nouns regardless of the point of interpolation, but interpolation relatively late in practice is more effective than interpolation relatively early (L. O. Krueger, 1930; W. C. F. Krueger, 1930). An alternation of readings and recalls without prompting is more effective than other groupings when the measure is immediate recall (Skaggs, 1920).

When recitations in the form of multiple choice tests on the material learned are introduced at some time after formal practice has ceased, their effectiveness decreases with the time between the criterion and the test (Spitzer, 1939; Sones and Stroud, 1940). This is to be expected, since the longer the interval, the more will the subject have forgotten and the less will there be to refresh by the recall.

(3) *The material learned.* The influence of recitation has been found to be greater with disparate and less meaningful materials than with connected and more meaningful ones. This is partly accounted for by the difficulty of maintaining clear experimental differentiation between reading and recitation when the material is meaningful and connected. The subject tends to think back over such material and to organize it as he goes. To this should be added the fact that less meaningful, disparate material is less readily assimilated to the subject's associative organization and would be

expected to profit relatively more from any condition which aids organization.

The method of recall with verbal materials is similar to symbolic or "imaginary" practice of activities which involve more motor manipulation. Symbolic practice of such activities is effective, but often less so than direct practice (Perry, 1939).[21]

Why Is Recall During Practice Effective?

It is probable that the experiments have seldom compared "pure" reading or presentation with recitation. Instead, the readings may have involved some recitation, so that the comparisons have been between small amounts of recitation and larger ones. The reports of the subjects and observation of their behavior permit a statement of some of the basic variables underlying the effectiveness of the relatively larger amount of recall during practice.

(1) Recitation furnishes the subject with progressive knowledge of results. This information (a) acts as an incentive condition, (b) brings the law of effect directly to bear, (c) favors early elimination of wrong responses, and (d) by informing the subject which items have been learned, promotes a more effective distribution of effort over the material.

(2) Recitation favors articulation of the items and leads to the utilization of accent and rhythm. (3) It likewise promotes grouping of the items, localization in the series, and the search for meaningful connections. (4) In recitation, the subject is practicing the material more nearly in the way in which it is to be tested and used—that is, without direct stimulation from the copy. It constitutes, therefore, a more immediately relevant form of practice.

[21] The effectiveness of recitation varies with a number of other conditions which will not be elaborated. For example, it seems to be less advantageous with very young children than with older ones. The young subjects spend too much time trying to recall items not yet fixated and are less able to take advantage of the method.

REFERENCES

Achilles, E. M. Experimental studies in recall and recognition. *Arch. Psychol., N.Y.*, 1920, 6, No. 44.

Adams, H. F. A note on the effect of rhythm on memory. *Psychol. Rev.*, 1915, 22, 289-298.

Adams, H. F., and Dandison, B. Further experiments on the attention value of size and repetition in advertisements. *J. appl. Psychol.*, 1927, 11, 483-489.

Balaban, A. Über den Unterschied des logischen und des mechanischen Gedächtnisses. *Z. Psychol.*, 1910, 56, 356-377.

Balken, E. R. Affective, volitional and galvanic factors in learning. *J. exp. Psychol.*, 1933, 16, 115-128.

Barker, R. G. A temporal finger maze. *Amer. J. Psychol.*, 1931, 43, 634-636.

Barlow, M. C. The role of articulation in memorizing. *J. exp. Psychol*, 1928, 11, 306-312.

Barton, J. W. Smaller vs. larger units in learning the maze. *J. exp. Psychol.*, 1921, 4, 418-429.

Beeby, C. E. An experimental investigation into the simultaneous constituents in an act of skill. *Brit. J. Psychol.*, 1930, 20, 336-353.

Bills, A. G., and Brown, C. The quantitative set. *J. exp. Psychol.*, 1929, 12, 301-323.

Brandt, E. R. The memory value of advertisements, with special reference to the use of color. *Arch. Psychol., N. Y.*, 1925, 13, No. 79.

Brown, R. W. The relation between two methods of learning piano music. *J. exp. Psychol.*, 1933, 16, 435-441.

Brown, W. Spatial integrations in a human maze. *Univ. Calif. Publ. Psychol.*, 1932, 5, 123-134.

Calhoon, S. W. A comparison of ability to reproduce one-syllable words and digits auditorially presented. *J. exp. Psychol.*, 1935, 18, 621-632.

Carlson, H. B., and Carr, H. A. Visual and vocal recognition memory. *J. exp. Psychol.*, 1938, 23, 523-530.

Carlson, H. B., and Carr, H. A. Rote and logical recognition memory. *J. exp. Psychol.*, 1940, 26, 199-210.

Carr, H. A. *Psychology: a study of mental activity.* New York: Longmans, Green, 1925.

Carter, H. D. Emotional correlates of errors in learning. *J. educ. Psychol.*, 1936, 27, 55-67.

Carter, H. D., Jones, H. E., and Shock, N. W. An experimental study of affective factors in learning. *J. educ. Psychol.*, 1934, 25, 203-215.

Cason, H. The learning and retention of pleasant and unpleasant activities. *Arch. Psychol., N. Y.*, 1932, 21, No. 134.

Cason, H. Associations between the familiar and the unfamiliar. *J. exp. Psychol.*, 1933, 16, 295-305.

Cofer, C. N. A comparison of logical and verbatim learning of prose passages of different lengths. *Amer. J. Psychol.*, 1941, 54, 1-20.

Cohn, J. Experimentelle Untersuchungen über das Zusammenwirken des akustisch-motorischen und des visuellen Gedächtnisses. *Z. Psychol.*, 1897, 15, 161-183.

Cook, T. W. Factors in whole and part learning a visually perceived maze. *J. genet. Psychol.*, 1936, 49, 3-31.

Cook, T. W. The relation between amount of material and difficulty of prob-

lem solving. I. Mental addition and subtraction. *J. exp. Psychol.*, 1937a, 20, 178-183.

Cook, T. W. Amount of material and difficulty of problem solving. II. The disc transfer problem. *J. exp. Psychol.*, 1937b, 20, 288-296.

Cook, T. W. Whole versus part learning the spider maze. *J. exp. Psychol.*, 1937c, 20, 477-494.

Cook, T. W. Distribution of practice and size of maze pattern. *Brit. J. Psychol.*, 1937d, 27, 303-312.

Cook, T. W. Guidance and transfer in part and whole learning of the disc transfer problem. *J. educ. Psychol.*, 1939a, 30, 303-308.

Cook, T. W. Repetitive pattern in whole and part learning the spider maze. *J. exp. Psychol.*, 1939b, 24, 530-541.

Cook, T. W., Morrison, S. H., and Stacey, C. L. Whole and part learning of a visually perceived maze. *J. genet Psychol.*, 1935, 47, 218-231.

Crafts, L. W. Whole and part methods with non-serial reactions. *Amer. J. Psychol.*, 1929, 41, 543-563.

Crafts, L. W. Whole and part methods with unrelated reactions. *Amer. J. Psychol.*, 1930, 42, 591-601.

Crafts, L. W. Whole and part methods with visual and spatial material. *Amer. J. Psychol.*, 1932, 44, 526-534.

Elkin, D. Über den Einfluss des Rhythmus und des Tempos auf den Gedächtnisprozess. *Arch. ges. Psychol.*, 1928, 64, 81-92.

Forlano, G. School learning with various methods of practice and rewards. *Teach. Coll. Contr. Educ.*, 1936, No. 688.

Foucault, M. Les inhibitions internes de fixation. *Année psychol.*, 1928, 29, 92-112.

Gates, A. I. The mnemonic span for visual and auditory digits. *J. exp. Psychol.*, 1916, 1, 393-403.

Gates, A. I. Recitation as a factor in memorizing. *Arch. Psychol., N. Y.*, 1917, 6, No. 40.

Gordon, K. Meaning in memory and in attention. *Psychol. Rev.*, 1903, 10, 267-283.

Guilford, J. P. The role of form in learning. *J. exp. Psychol.*, 1927, 10, 415-423.

Guilford, J. P., and Dallenbach, K. M. The determination of memory span by the method of constant stimuli. *Amer. J. Psychol.*, 1925, 36, 621-628.

Hanawalt, E. M. Whole and part methods in trial and error learning. *Comp. Psychol. Monogr.*, 1931, 7, No. 35.

Hanawalt, E. M. Whole and part methods in trial and error learning: a supplementary study. *J. comp. Psychol.*, 1933, 15, 395-406.

Hanawalt, E. M. Whole and part methods in trial and error learning: human maze learning. *J. exp. Psychol.*, 1934, 17, 691-708.

Harding, D. W. Rhythmization and speed of work. *Brit. J. Psychol.*, 1933, 23, 262-278.

Henmon, V. A. C. The relation between mode of presentation and retention. *Psychol. Rev.*, 1912, 19, 79-96.

Hovland, C. I. Experimental studies in rote-learning theory. VII. Distribution of practice with varying lengths of list. *J. exp. Psychol.*, 1940, 27, 271-284.

Hull, C. L., *et al. Mathematico-deductive theory of rote learning: a study in scientific methodology.* New Haven: Yale Univ. Press, 1940.

Husband, R. W. Human learning on a four-section, elevated, finger maze. *J. gen. Psychol.*, 1928, 1, 15-28.

Husband, R. W. Analysis of methods in human maze learning. *J. genet. Psychol.*, 1931, 39, 258-277.

Jones, M. G., and English, H. B. Notional vs. rote memory. *Amer. J. Psychol.*, 1926, 37, 602-603.

Karwoski, T. The memory value of size. *J. exp. Psychol.*, 1931, 14, 539-554.

Katona, G. *Organizing and memorizing: studies in the psychology of learning and teaching.* New York: Columbia Univ. Press, 1940.

Key, C. B. Recall as a function of perceived relations. *Arch. Psychol., N. Y.*, 1926, 13, No. 83.

Klemm, O., and Olsson, E. Über den Einfluss mechanischer und sinnvoller Hilfen bei Gedächtnisleistungen. *Z. pädag. Psychol.*, 1925, 26, 188-194.

Koch, H. L. A neglected phase of the part-whole problem. *J. exp. Psychol.*, 1923, 6, 366-376.

Koch, H. L. Some factors affecting the relative efficiency of certain modes of presenting material for memorizing. *Amer. J. Psychol.*, 1930, 42, 370-388.

Krueger, L. O. The relative effect of interspersing a recall at different stages of learning. *Arch. Psychol., N. Y.*, 1930, 18, No. 114, 15-25.

Krueger, W. C. F. The optimal effect of a recall during learning. *Arch. Psychol., N. Y.*, 1930, 18, No. 114, 26-34.

Lyon, D. O. The relation of length of material to time taken for learning and the optimum distribution of time. *J. educ. Psychol.*, 1914, 5, 1-9; 85-91; 155-163.

Lyon, D. O. *Memory and the learning process.* Baltimore: Warwick and York, 1917.

McGeoch, G. O. The intelligence quotient as a factor in the whole-part problem. *J. exp. Psychol.*, 1931a, 14, 333-358.

McGeoch, G. O. Whole-part problem. *Psychol. Bull.*, 1931b, 28, 713-739.

McGeoch, J. A. The influence of associative value upon the difficulty of non-sense-syllable lists. *J. genet. Psychol.*, 1930, 37, 421-426.

McGeoch, J. A., and McKinney, F. Studies in retroactive inhibition. VIII. The influence of the relative order of presentation of original and interpolated paired associates. *J. exp. Psychol.*, 1937, 20, 60-83.

McGeoch, J. A., and Oberschelp, V. J. The influence of length of problem and of transfer upon rational learning and its retention. *J. gen. Psychol.*, 1930, 4, 154-168.

McGeoch, J. A., and Whitely, P. L. The recall of observed material. *J. educ. Psychol.*, 1926, 17, 419-425.

Miles, W. R. The high relief finger maze for human learning. *J. gen. Psychol.*, 1928, 1, 3-14.

Miles, W. R. Unidirectional mazes for studying human learning. *J. gen. Psychol.*, 1931, 5, 269-275.

Moore, H. T. The attention value of lecturing without notes. *J. educ. Psychol.*, 1919, 10, 467-469.

Müller, G. E. Zur Analyse der Gedächtnistätigkeit und des Vorstellungsverlaufes. *Z. Psychol.*, 1911, 1913, 1917, Ergbd. 5, 8, 9.

Müller, G. E., and Schumann, F. Experimentelle Beiträge zur Untersuchung des Gedächtnisses. *Z. Psychol.*, 1894, 6, 81-190; 257-339.

Myers, C. S. *A text-book of experimental psychology.* (3rd ed., Pt. I) New York: Longmans, Green, 1925.

Newhall, S. M., and Heim, M. H. Memory value of absolute size in magazine advertising. *J. appl. Psychol.*, 1929, 13, 62-75.

Nyswander, D. B. A comparison of the high relief finger maze and the stylus maze. *J. gen. Psychol.*, 1929, 2, 273-288.

Offner, M. *Das Gedächtnis. Die Ergebnisse der experimentellen Psychologie und ihre Anwendung in Unterricht und Erziehung.* Berlin: Reuther and Reichard, 1909 (1924).

Pechstein, L. A. Whole vs. part methods in motor learning. A comparative study. *Psychol. Monogr.*, 1917, 23, No. 99.

Pechstein, L. A. Whole versus part methods in learning nonsensical syllables. *J. educ. Psychol.*, 1918, 9, 381-387.

Pechstein, L. A. The whole vs. part methods in learning: comparison and summary. *Stud. Educ. (Yearb. nat. Soc. Coll. Teach. Educ.)*, 1926, 15, 181-186.

Perry, H. M. The relative efficiency of actual and "imaginary" practice in five selected tasks. *Arch. Psychol., N. Y.*, 1939, 34, No. 243.

Peters, H. N. The judgmental theory of pleasantness and unpleasantness. *Psychol. Rev.*, 1935, 42, 354-386.

Peterson, H. A. On the influence of complexity and dissimilarity on memory. *Psychol. Monogr.*, 1909, 12, No. 49.

Peterson, J. Experiments in rational learning. *Psychol. Rev.*, 1918, 25, 443-467.

Pialat, E. Recherches sur la fonction mnémonique de la forme et de la couleur dans les images colorées. *J. Psychol.*, 1929, 26, 101-121.

Reed, H. B. Associative aids. I. Their relation to learning, retention, and other associations. II. Their relation to practice and the transfer of training. *Psychol. Rev.*, 1918, 25, 128-155; 257-285.

Reed, H. B. Part and whole methods of learning. *J. educ. Psychol.*, 1924a, 15, 107-115.

Reed, H. B. Repetition and association in learning. *Ped. Sem.*, 1924b, 31, 147-155.

Reed, H. B. Meaning as a factor in learning. *J. educ. Psychol.*, 1938, 29, 419-430.

Robinson, E. S., and Heron, W. T. Results of variations in length of memorized material. *J. exp. Psychol.*, 1922, 5, 428-448.

Rubin-Rabson, G. Studies in the psychology of memorizing piano music. III. A comparison of the whole and the part approach. *J. educ. Psychol.*, 1940, 31, 460-476.

Sand, M. C. The effect of length of list upon retroactive inhibition when degree of learning is controlled. *Arch. Psychol., N. Y.*, 1939, 33, No. 238.

Sawdon, E. W. Should children learn poems in "wholes" or in "parts"? *Forum Educ.*, 1927, 5, 182-197.

Scott, T. C., and Henninger, L. L. The relation between length and difficulty in motor learning; a comparison with verbal learning. *J. exp. Psychol.*, 1933, 16, 657-678.

Seagoe, M. V. Qualitative wholes: a re-valuation of the whole-part problem. *J. educ. Psychol.*, 1936a, 27, 537-545.

Seagoe, M. V. The influence of degree of wholeness on whole-part learning. *J. exp. Psychol.*, 1936b, 19, 763-768.

Seagoe, M. V. Additional laboratory experiments with qualitative wholes. *J. exp. Psychol.*, 1937, 20, 155-168.

Sharp, A. A. An experimental test of Freud's doctrine of the relation of hedonic tone to memory revival. *J. exp. Psychol.*, 1938, 22, 395-418.

Shurrager, H. C. The measurement of memory on an absolute scale. *Psychol. Monogr.*, 1940, 52, No. 234, 21-38.

Skaggs, E. B. The relative value of grouped and interspersed recitations. *J. exp. Psychol.*, 1920, 3, 424-446.

Skaggs, E. B., and Grossman, S. The optimum number of readings before introducing reading prompting in verbatim learning. *Arch. Psychol., N. Y.*, 1930, 18, No. 114, 5-14.

Smedley, F. W. *Report of the Department of Child Study and Pedagogic Investigation, 1900-1901*, No. 3. Chicago: Blakely, 1902.

Smith, M. K. On the reading and memorizing of meaningless syllables presented at irregular time intervals. *Amer. J. Psychol.*, 1907, 18, 504-513.

Snygg, D. The relative difficulty of mechanically equivalent tasks. I. Human learning. *J. genet. Psychol.*, 1935, 47, 299-319.

Sones, A. M., and Stroud, J. B. Review, with special reference to temporal position. *J. educ. Psychol.*, 1940, 31, 665-676.

Spitzer, H. F. Studies in retention. *J. educ. Psychol.*, 1939, 30, 641-656.

Stagner, R. Factors influencing the memory value of words in a series. *J. exp. Psychol.*, 1933, 16, 129-137.

Steffens, L. Experimentelle Beiträge zur Lehre vom ökonomischen Lernen. *Z. Psychol.*, 1900, 22, 321-382.

Stoke, S. M. Memory for onomatopes. *J. genet. Psychol.*, 1929, 36, 594-596.

Terman, L. M. *The measurement of intelligence: an explanation of and a complete guide for the Stanford Revision and extension of the Binet-Simon Intelligence Scale.* Boston: Houghton Mifflin, 1916.

Terman, L. M., and Merrill, M. A. *Measuring intelligence.* Boston: Houghton Mifflin, 1937.

Thorndike, E. L., *et al. The fundamentals of learning.* New York: Teachers College, Columbia Univ., 1932.

Thurstone, L. L. A method of scaling psychological and educational tests. *J. educ. Psychol.*, 1925, 16, 433-451.

Thurstone, L. L. The relation between learning time and length of task. *Psychol. Rev.*, 1930, 37, 44-53.

Van Buskirk, W. L. An experimental study of vividness in learning and retention. *J. exp. Psychol.*, 1932, 15, 563-573.

Warden, C. J. Primacy and recency as factors in cul-de-sac elimination in a stylus maze. *J. exp. Psychol.*, 1924a, 7, 98-116.

Warden, C. J. The relative economy of various modes of attack in the mastery of a stylus maze. *J. exp. Psychol.*, 1924b, 7, 243-275.

Warden, C. J. The factor of movement in the presentation of rote memory material. *Amer. J. Psychol.*, 1926, 37, 257-260.

Winch, W. H. Should poems be learnt by school-children as "wholes" or in "parts"? *Brit. J. Psychol.*, 1924, 15, 64-79.

Winzen, K. Die Abhängigkeit der paarweisen Assoziation von der Stellung des besser haftenden Gliedes. *Z. Psychol.*, 1921, 86, 236-252.

Woodrow, H. The measurement of difficulty. *Psychol. Rev.*, 1936, 43, 341-365.

Woodworth, R. S. A contribution to the question of "quick learning, quick forgetting." (Abstract.) *Psychol. Bull.*, 1914, 11, 58-59.

Woodworth, R. S. *Experimental psychology.* New York, Holt, 1938.

Wylie, G. E. Whole vs. part methods of learning as dependent upon practice. Ph.D. Dissertation, Univ. Chicago, 1928.

XII

INDIVIDUAL DIFFERENCES AND LEARNING

INTRODUCTION

VARIABLES such as the interval between practice trials or the length of the material learned may be controlled and manipulated independently of the subject, while the variables of age, sex, and intelligence are direct characteristics of the learner. When we ask how rate of learning varies with the number of years one has lived, with one's level of mental development, or with one's sex, we are asking whether and to what extent learning is a function of these characteristics. Their biological and social importance gives the answer special significance. The differences obtained are individual differences and appear when interval between trials, amount of material, and similar conditions are constant.

CHRONOLOGICAL AGE

As the inescapable individual variable of chronological age increases from infancy to old age, how does rate of learning change? Early in the history of research on learning this question was asked about a more restricted age range, and work was confined largely to the ages 6 or 8 to 18 or 20. Not only were they regarded as the important years for learning in school, but subjects within this range of ages were readily obtainable. Investigation over the years from birth to 6 or 8 and from 18 or 20 to senescence has reached important proportions only recently, when the learning of infants and young children has begun to be studied as part of experimental child psychology [1] and when interest in adult education, in certain problems of industrial personnel, and in the social effectiveness of

[1] The extent to which learning pervades child behavior is recognized in Buford Johnson's statement (1932) that "child psychology is primarily a study of learning."

older people has called attention to a need for information about the rates of learning at chronological ages beyond early maturity (cf. Bird, 1940).

Age, in the sense of the length of time lived by an individual, cannot itself be a determiner of learning. Time, in and of itself, does nothing, but is scientifically important only as an index of other variables which are correlated with it or, in more figurative terms, because it is the bearer of other conditions. When we use age as an independent experimental variable, we regard it as a useful symbol for the complex conditions which age brings with it.

The method commonly employed in studies of age-learning relations is that of measuring the rates of learning of representative samples of subjects at each age over a band of ages. This method of *successive cross-sectional sampling* tells us the average rate of learning of samples of subjects from successive ages. It permits us to conclude that the average 12-year-old in the population sampled learns faster than the average 6-year-old, or that the average person of CA 60 learns more slowly than the average person of CA 20 in a similar population. This method, of course, suffers from the disadvantage that it is subject to various forms of bias or non-random sampling error. Changes in the characteristics of the population as a whole or the population of a particular geographical section may have an influence upon the results obtained. Similarly, the younger groups may have been subjected to a systematically different type of education from the older groups. Furthermore, if the ability to learn serves an adaptive function in promoting survival of the individuals who possess it to a high degree, biological selection (of unmeasured amount) may influence the results.

An alternative method would be to select a representative sample of subjects at some early age and to measure the rate of learning of each member of the sample annually or oftener over a long period of years. This *longitudinal* method is theoretically desirable, but practically difficult. Its use would require a large section of the productive life of an investigator; a group of subjects selected early in life would be difficult to follow systematically; and the number of equivalent sections of learning material required would be very

difficult to obtain. The major experiments on the relations between age and learning have been performed by the method of successive cross-sectional sampling.

The Period from Birth to Early Maturity

Learning during Infancy

Studies of infants have demonstrated, and close observers have long known, that changes in performance which seem to satisfy any useful definition of learning begin to appear at, or very soon after, birth. There have also been some more or less reasonable speculations concerning intra-uterine learning, and there is some evidence of fetal conditioning. Reflexes and simple sensori-motor forms of behavior show, during the first few hours of extra-uterine life, changes which resemble closely those of the quantitative learning curves, although the relative contributions of practice and maturation are difficult to isolate. Simple movements and emotional responses may be conditioned early; the learning of simple social responses soon appears; and with the beginning of walking and talking a bewildering complexity of learned responses, including simple concept formation, presents itself. There is evidence also that conditioned responses are established with increasing rapidity as age increases up to CA 6.[2]

The data leave little room for doubt that learning occurs from the hour of birth onward, but they do not permit systematic comparisons of the rates of learning of children of different ages. The data upon what young children can and do learn are not the important ones here, but, rather, the results which yield comparisons at different ages over relatively wide bands of ages. The majority of such data are for the ages beyond CA 6. If we are to make direct comparisons

[2] Hollingworth's (1928) article presents some interesting possibilities concerning intra-uterine learning. The papers by Ray (1932) and Spelt (1948) report studies of fetal conditioning. Those by Mateer (1918), D. P. Marquis (1931, 1941), Wenger (1936), and Kantrow (1937) contain illustrative work on learning by young infants. A survey of the literature up to 1933 will be found in the chapter by Peterson, entitled "Learning in Children," in Murchison's *Handbook* (1933). References to some of the research on concept formation by young children are given by Welch (1940) and in the papers he cites.

between rates of learning at different ages, the subjects at each age should practice the same material under the same conditions. This requirement cannot be met readily during the period of infancy and very early youth. After CA 6 to 8, however, samples from each age may be tested on memory span or an immediate reproduction of a poem studied for a constant time, may practice verbal materials or perceptual-motor acts to a criterion, or may be given rational problems to solve, and direct comparisons may legitimately be made. The results will be presented under the head of the classes of materials learned.

Lack of systematic information concerning learning during the period of infancy and early childhood should not, however, obscure the importance of the fact that this learning can and does occur. The fact that the human organism is capable of learning before birth (but probably learns little in utero under ordinary circumstances) and that he shows signs of adapting himself to his environment through learning during the days immediately following his birth, is of enormous psychological significance. It is highly probable that many basic personality patterns are acquired through learning during the first few years of life. Certainly, an enormous number of instrumental acts are acquired during this period. The psychoanalytic school, of course, has placed especial emphasis upon this important point, and it is unfortunate that relevant experimental data do not exist in greater quantity. On the other hand, the significance of early learning may have been overemphasized in some cases. For example, it would be unwise at our present state of knowledge to interpret the demonstration of fetal conditioning as evidence which strongly supports the theories of Rank (1929) to the effect that personality is, to a considerable extent, determined by one's memory of being born.

VERBAL MATERIALS

The number of papers on memory span performance at different ages has been large and, in spite of the fact that the conditions of the experiments have differed widely, the results agree remarkably. They show that the immediate reproduction called for in memory

span performance increases slowly, but certainly, from an early age to the ages 10 to 12 and somewhat more slowly from that region to CA 18 or slightly beyond. The form of the age-learning relation over the total range is best represented by a decelerated curve. Memory span performance has been measured as early as CA 2, from which point onward increments with age appear. At CA 5 the reported digit spans are in the vicinity of 4, increasing to 7 or 8 by adulthood. The amount of the increase from year to year is small, but the upward trend is unmistakable.[3]

Analogous results appear when the amount of disparate material presented is beyond the subject's span and when measurement is in terms of whatever fraction of this amount he can recall. The materials and conditions have been diverse, but again an upward trend is clear. It occurs with lists of words of different classes and meanings (Netschajeff, 1900; Pyle, 1913, 1928), with geometrical forms and nonsense syllables (Mulhall, 1917; Achilles, 1920; Stroud and Maul, 1933), with perceptual materials both meaningful and meaningless (Pilek, 1932), and with many others, and extends over the range from about CA 8 to CA 16 to 18.

Connected materials, such as poetry and prose, whether learned verbatim or for the component ideas, are more nearly like the materials which people learn in everyday life or in school, and here the results are at least as convincing as are those with disparate materials. In an experiment by Stroud and Maul (1933) the mean number of lines of poetry immediately recalled after 15 minutes of study increases steadily with CA from a mean age of 7.7 to one of 18.1 (Table XXXIV). Comparisons of means of widely separated age groups show reliable differences, although the differences between successive age groups are not statistically reliable.

Similar increments with age appear when the ideational content of prose, studied for a constant time by all subjects, is recalled

[3] Illustrative evidence on the relation of CA and memory span will be found in the early work of Jacobs (1887), in the work of Humpstone (1917) and Starr (1923) with relatively extensive age ranges, in a study by Gundlach, Rothschild, and Young (1927) of a phenomenon that they call set, but that is also related to span performance, in Hallowell's (1928) study of the age range 1:8 to 3:11, and in Hurlock and Newmark's (1931) measurements of the spans of preschool children.

either freely or in answer to specific questions. Pyle's (1913) data upon immediate reproduction of ideas in a meaningful paragraph show a uniform increase from CA 8 to CA 13 for males and CA 15 for females. An inversion occurs after these ages, but at adulthood the mean is higher for each sex than at any preceding age. The departure from the trend of the curve after the ages mentioned is probably a function of sampling errors and does not signify a representative decrement in performance. The years beyond about CA 14 are difficult ones at which to obtain a representative sampling of subjects.

TABLE XXXIV

IMMEDIATE RECALLS OF POETRY AND NONSENSE SYLLABLES AS A FUNCTION OF AGE

(From Stroud and Maul, *J. genet. Psychol.*, 1933, 42, p. 244)

Average CA	Average IQ	Lines of Poetry Recalled M	P.E.	Number of Syllables Recalled M	P.E.
7.7	115	9.71	.54	4.73	.19
8.5	115	11.16	.43	5.12	.21
9.4	115	13.15	.47	5.82	.23
10.4	111	16.02	.56	6.43	.24
11.7	103	17.55	.65	6.74	.24
14.4	109	21.31	.89	7.39	.30
18.1	114	22.14	.65	8.71	.39

Pyle (1925a) has also tested amount learned by asking a series of prepared questions on the prose read. The subjects were under instruction to learn the ideas expressed, not the specific words, and different sets of material were employed, each one adapted to a different band of ages. When the records for all materials are converted into a single set of scores, the curves of Figure 56, which are representative of the general trend of recall with age, are obtained. The records for the two sexes, which do not differ greatly from each other, combine to give nearly a straight line. The failure of deceleration to appear in the combined curve may be a function either of the conditions of the experiment or of Pyle's method of com-

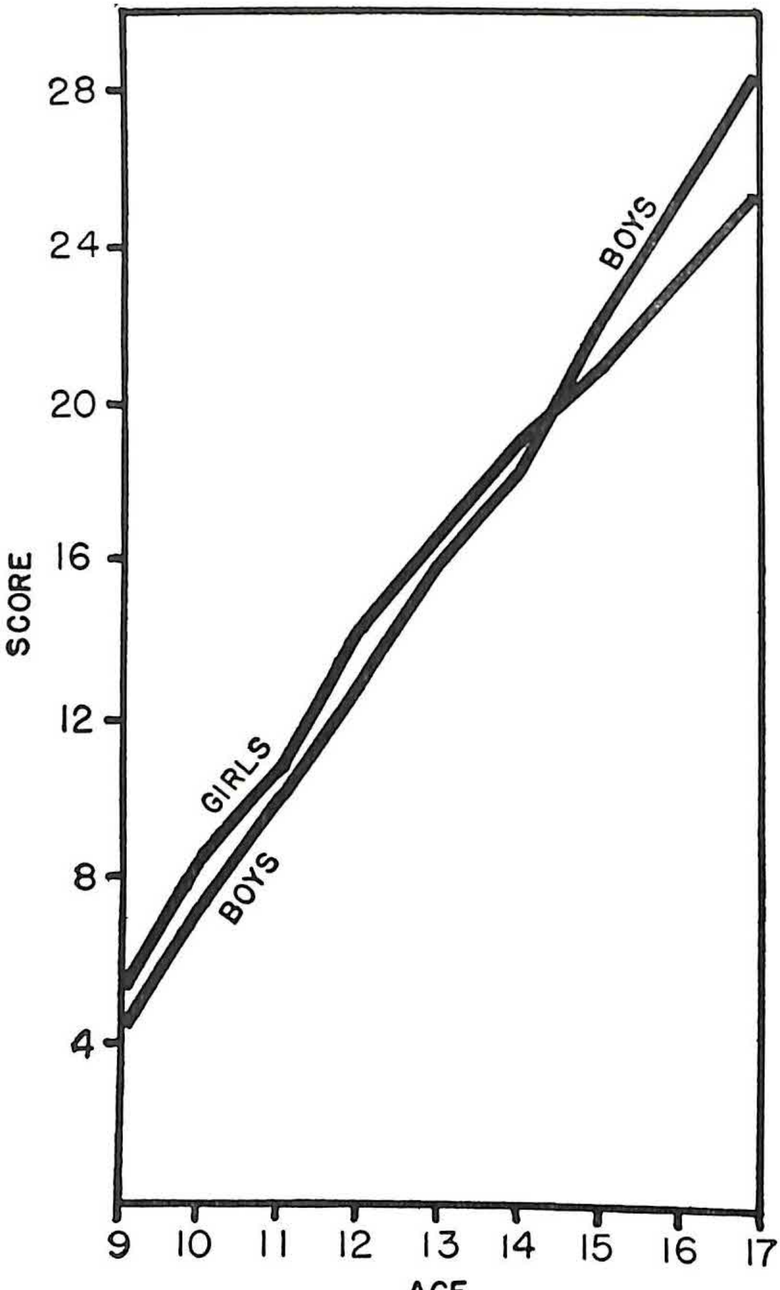

FIG. 56. RELATIONS BETWEEN CHRONOLOGICAL AGE AND SCORES IN IDEATIONAL LEARNING

(From Pyle, *Nature and development of learning capacity*, p. 79)

bining scores from different sets of material. However this may be, there is no doubt of the increasing recall with increasing age.

A somewhat different approach to the problem is afforded by work employing the *Aussage* method. Here the subject is presented with a complex perceptual field, with or without instruction to study it for reproduction, is allowed to observe it for a relatively brief time, and is then asked to reproduce it or to *report* on it. The report may take the form of a narrative in which he gives as complete an account as possible or of an interrogation in which he answers a series of specific questions. The most frequently used materials have been cards containing a number of objects, pictures, and somewhat dramatic events. Again, immediate recall increases with increasing age (McGeoch, 1928a).

Studies of the number of trials required to learn to a criterion have been less numerous than the studies of immediate memory. Results such as those of Goldscheider (1932), however, are supported by a number of more incidental observations. His method of alternating presentations and tests is far from optimal because, among other difficulties, it gives an advantage to subjects with relatively higher initial performance, but we may accept the general tendencies of the obtained relations. The number of trials required to learn six syllables decreases steadily from 12.6 at CA 9 to 3.5 at CA 13, while over the same band of ages the trials to learn six words decrease irregularly from 5.5 to 2.4, and the trials to learn six numbers from 3.4 to 1.4. The closeness of the number of items used to the memory span of many of the subjects probably accounts for the relatively smaller and more irregular increases with age in learning words and numbers.

PERCEPTUAL-MOTOR ACTIVITIES

It has been established that the rates of learning of a considerable number of perceptual-motor acts increase with chronological age. Hicks and Carr (1912) found that adult graduate students required fewer errors and less time and traversed a smaller amount of excess distance in learning a maze through which they actually walked

blindfolded than did children of CA 8 to 13. This conclusion has been corroborated by Gould and Perrin (1916) with a complex stylus maze in which children made more errors and traveled more excess distance than adults, and by McGinnis (1929) with a slot maze practiced for 50 trials by children aged 3, 4, and 5. The time taken on the first five trials decreases as age increases, and this relation obtains at each block of five throughout the experiment. The same is true for errors, save for certain minor exceptions.

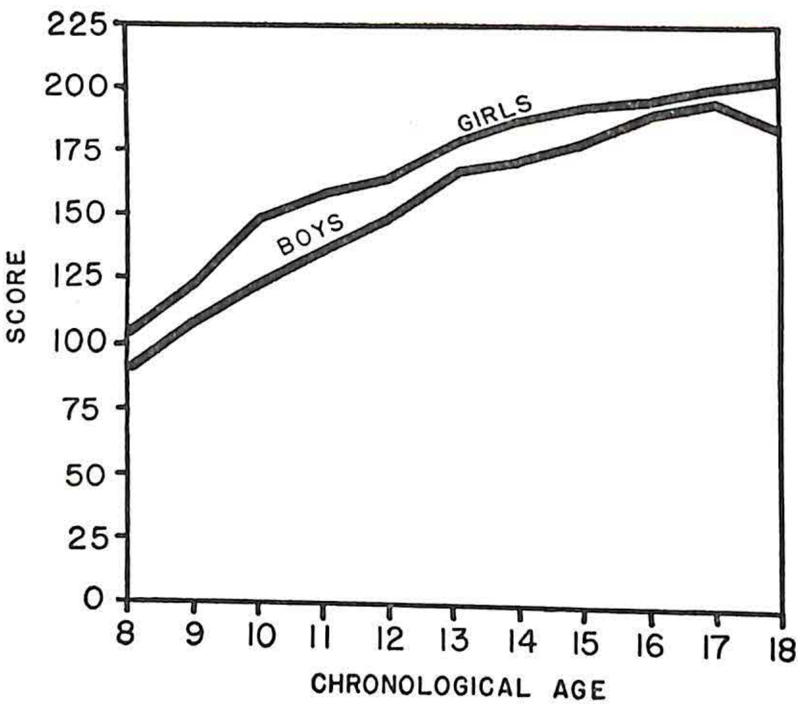

FIG. 57. RELATIONS BETWEEN AGE AND SCORE IN CARD-SORTING (From Pyle, *Nature and development of learning capacity*, data on p. 16)

Pyle (1925a) has gathered a large amount of data upon the performance of different age groups in card-sorting, simple and complex marble-sorting, digit-symbol substitution, and mirror-drawing. The curves for card-sorting and substitution given in Figures 57 and 58 have a clear upward trend with increasing CA. The decrease in the performance of the boys from CA 17 to 18 is almost certainly a function of sampling error. The curves for the other activities

which Pyle studied reveal similar relations between CA and performance. Those for simple marble-sorting and mirror-drawing fail to show any marked deceleration in the later years of adolescence.

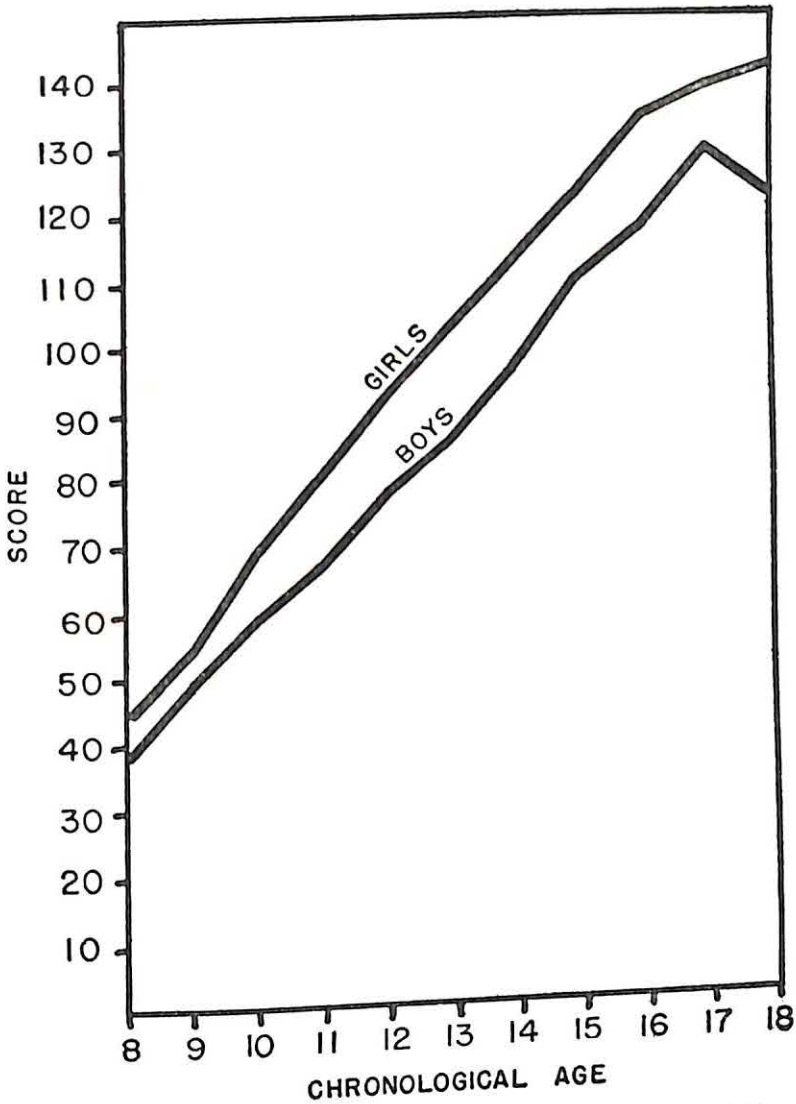

FIG. 58. RELATIONS BETWEEN AGE AND SCORE IN SUBSTITUTION
(From Pyle, *Nature and development of learning capacity*, data on p. 45)

This may be largely accounted for by the particular conditions of the experiment, which do not require discussion here. Positive relations between CA and records of learning have been reported for

piano lessons (Brown, 1936), for operating a pursuitmeter (Langhorne, 1933), and for a considerable number of other perceptual-motor activities.

One must concludes that practice effects on a wide range of perceptual-motor acts increase with age from about CA 8 to the years of late adolescence or early maturity. The measures of practice effect have usually been composites of initial performance and gain from specific practice. Only the latter is strictly a measure of rate of learning, since initial status is probably more heavily weighted by transfer from prior learning, done at unknown rates, to the first or early trials of the activity of the experiment. It is often a question, therefore, whether to call the results measures of rate of learning or of transfer plus rate of learning, and usually the latter is the more accurate.

LEARNING WHICH INVOLVES GRASPING OF RELATIONS

Studies of relations between CA and the solution of relational problems have been less extensive than those of the relations between CA and learning of verbal materials or of perceptual activities, but their implications have been the same. Heidbreder (1928), for example, investigated the solution of simple problems of the multiple-choice type by children aged 3, 4, and 6 to 10, and by university students, presenting each subject with the problem of discovering the principle which would permit an errorless choice among the alternatives presented. The data afford a picture of increasing rate of problem-solving with increasing age. A number of other investigators have reported an increase with age in ideational problem-solving and in the learning of abstract concepts.

COEFFICIENTS OF CORRELATION BETWEEN AGE AND RATE OF LEARNING

If coefficients of correlation had been computed between age and the data from which the preceding conclusions were drawn, they would have shown a positive relation. Many investigators have not computed them, and unless one desires to know the mutual

interrelations of the two variables it is not necessary to do so. The more usual curves, which are regression lines of learning upon age, satisfy our major interest with respect to the problem, since we are concerned with knowing how learning varies with age, not how age varies with learning. The picture yielded by the coefficients is interesting, however, and a few characteristic samples from young subjects may be mentioned.

Taylor (1931) reports a correlation of 0.84 between age and immediate memory in 30 nursery-school children, Emerson (1931) one of 0.77 between age and score in immediate memory for the spatial placement of wooden rings on an easel, and Foster (1928) one of 0.74 between age and total score on the learning of eight stories. The correlations obtained by Knotts and Miles (1929) with normal subjects between CA and learning records on a finger maze and a stylus maze vary between −0.30 and −0.58, depending on the score, the coefficients being computed so that a negative coefficient means a positive relation between age and score on the maze.

The correlations between CA and measures of learning present the same general picture as do the curves. At the lower end of the age range the coefficients are relatively high; toward the middle of the range from birth to early maturity they are low, but still positive; while by early maturity they become low or inverse. These relations are roughly what one would expect from sections of a negatively accelerated age-learning curve. They are inevitably functions of the length of the band of ages sampled and, in view of the narrowness of the bands commonly used in correlational studies and of the other conditions known to be present and to attenuate coefficients of correlation, many of the coefficient obtained may be regarded as high.

The Period from Early Maturity to Old Age

Any division of the life span into periods is arbitrary and in danger of blurring the fundamental continuity of phenomena in the living organism. A distinction is made here between studies of the ages from birth to early maturity and those of the years beyond only

because the experimental work most readily classifies itself that way. It should be remembered that results from the two age intervals are to be regarded as continuous.

The most comprehensive study of learning during the years from early to late maturity was published by Thorndike in 1928. As an introduction to reports of his own experiments, he reviewed a large number of the available comparisons of youthful and adult learning. Most of the data were incidental and meager, and there is no need to deal with them here beyond a quotation of Thorndike's conclusions from them. He wrote:

> "(1) The differences in rate of learning between old and young are small in comparison with the differences within either group. (2) When other factors than age are equalized or 'partialed out,' the influence approaches zero. (3) If each author is given equal weight, we have an equal division among reports of superiority, inferiority, and equality to the adult learner. If we attach, as we should, more weight to the experiments which report learning during a long period or for many individuals, we get about the same balance. On the whole, if we did have to estimate on the basis of this chapter's reports, we should estimate adult ability to learn as very close to that of the late teens."

So much for the early and unsystematic work prior to 1928. The more recent and more systematic work has been done or directed by a few psychologists whose work has been extensive.

AGE AND IMMEDIATE RECALL

Research on the rates of learning of individuals during the years of later maturity faces the difficulty of obtaining adequate samples at these ages. Fortunately, H. E. Jones (1928) was able to obtain a sampling of 618 subjects which could be shown to be representative of the population of Vermont and, more notable still, included as good a selection at the higher age levels as at the lower. His data on the immediate recall of the content of moving pictures cover the age range from 10 to 60, recall being in response to completion and multiple choice tests. The tests have a relatively high reliability ($r = 0.89$ to 0.95) and, although the number of cases at each age

is not large, the results are probably a good approximation to those from a much larger sampling. Three separate films were used and different subjects, for the most part, observed and recalled each. The age curves for the three so clearly have the same general form that only the composite need be presented. It has been drawn from the smoothed sigma score equivalents of the raw scores (Fig. 59).

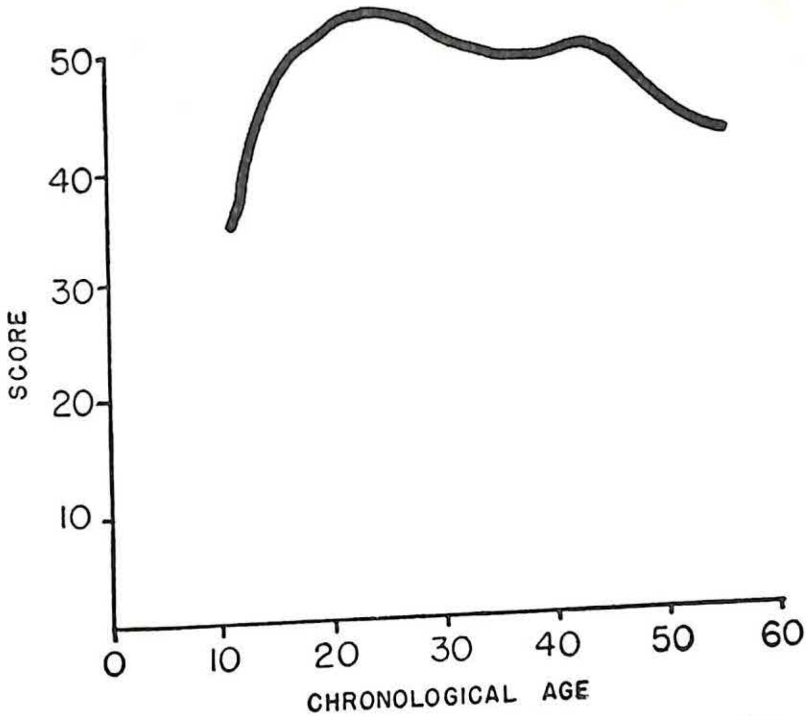

FIG. 59. COMPOSITE CURVE SHOWING THE RELATION BETWEEN RECALL OF THREE MOTION PICTURES AND CHRONOLOGICAL AGE
(From Jones, *Univ. Calif. Publ. Psychol.*, 1928, 3, pp. 238-239)

This curve shows over the years of youth and early maturity the negatively accelerated relation which many other studies of this period would lead us to expect. The maximal performance is reached at CA 23.5, from which point onward the curve falls. From CA 17 to CA 42, however, the largest difference between the scores at any two ages is very small. Across a band of twenty-five years, therefore, learning as measured by immediate recall of a motion picture declines only a little. The decrease from CA 42 onward is

regular and somewhat larger, although the performance of the oldest group used (mean CA 55.6) is still equal to that of subjects slightly past CA 12. The total curve thus rises to a peak in the early twenties, falls slowly and by amounts that are practically inconsequential from then until the early forties, and more rapidly from the early forties to the middle fifties, at which point it has reached a level roughly equivalent to the score of twelve-year-olds.

The subjects in Jones's study were not told that they were to be examined upon the contents of the pictures and, in one sense, therefore, his was a study of incidental learning or of learning without specific intent to learn. In an investigation by Willoughby (1929, 1930) the learning was also incidental. The subjects were asked

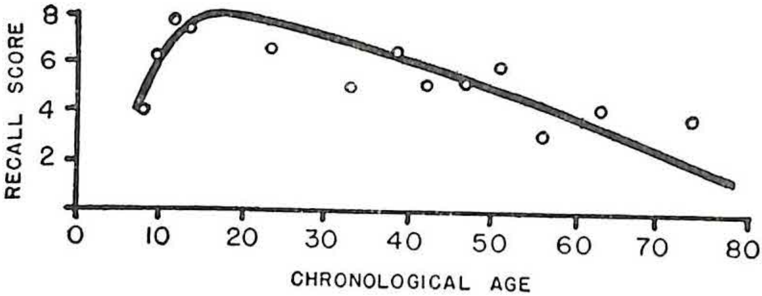

FIG. 60. A CURVE SHOWING THE RELATION BETWEEN RECALL OF DIGIT-SYMBOL ASSOCIATES AND CHRONOLOGICAL AGE
(From Willoughby, *J. educ. Psychol.*, 1929, 20, p. 678)

to recall the symbols in a digit-symbol substitution test at which they had been working for 2.5 minutes with the key before them, but without instruction to connect symbol with digit for the purposes of an independent recall. Willoughby employed 300 subjects distributed in class intervals of CA of two years each and ranging from 6 years to 68+. The number of cases in each interval ranged between 80 (in the 12.0 to 13.11 year group) to 1 (in the 68 + group). When recall of the symbols is plotted against age, the resulting curve (Fig. 60) has a sharp initial rise to a peak in the late teens and a decrease from that point onward. This curve must be taken to mean what the subjects actually did when they were tested on material which they had not been specifically instructed to learn for the purposes of

such a test; it does not directly show what they could do under specific motivation. It should also be noted that the older subjects had less practice than did the younger ones in terms of the number of correct substitutions. This is shown in Figure 61, which shows the score in digit-symbol substitution as a function of age. It will be seen that this curve closely parallels the one shown in Figure 60. Thus, it may be that Willoughby's data on recall demonstrate only that less practice yields a smaller amount of learning. On the other

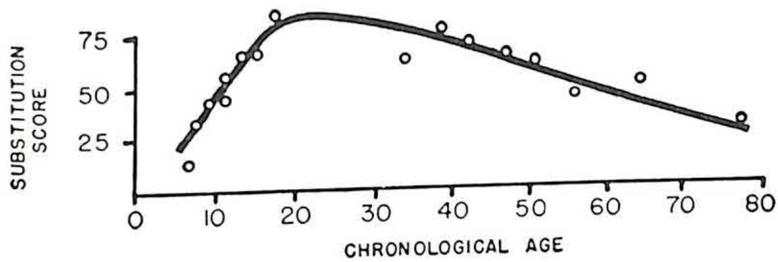

FIG. 61. A CURVE SHOWING THE RELATION BETWEEN SCORE IN DIGIT-SYMBOL SUBSTITUTION AND CHRONOLOGICAL AGE
(From Willoughby, *J. educ. Psychol.*, 1929, 20, p. 678)

hand, both amount of practice and amount of recall may be independent functions of CA. From Willoughby's results, no specific conclusions about the recall of incidentally learned material as a function of CA may be drawn.

PERCEPTUAL-MOTOR ACTIVITIES AND OTHERS

The curve of performance in substitution (Fig. 61) closely resembles that for recall of the symbols. It rises sharply to a mode in the vicinity of CA 22 and falls away toward old age. In one of Thorndike's (1928) experiments with substitution, 277 graduate students in Education worked for eight 3-minute periods transcribing disconnected words in terms of a code alphabet in which each letter of the alphabet stood for some other letter. The subjects were then asked, as in Willoughby's experiment, to recall the code which they had not been specifically told to learn. Whatever measure one considers, performance is seen to decrease with increasing age

(Table XXXV). Thorndike considers the average gains to give the best indication of the effect of the specific practice.

TABLE XXXV

SCORES MADE IN CODE SUBSTITUTION BY SUBJECTS OF DIFFERENT AGES

(From Thorndike, *Adult learning*, p. 86)

	Age Group 20-24	25-34	35+
Number of cases	28	139	104
Average age	23	29	41
Score, first 3′ period	59.8	56.0	47.3
Score, last 3′ period	87.8	83.9	70.1
Average gain	28.0±1.2	27.9±0.7	22.8±0.7 *
Average no. recalled	10.4±0.7	8.3±0.3	6.3±0.3

* The measures of variability are P.E.'s of the averages.

The extensive studies of later maturity by Miles (1933a, b; 1935) have included several measures of rate of learning over a wide range of CA. As an example, the average scores in alphabet-alphabet substitution for 5 minutes are at a high level over the class intervals CA 18–29 and 30–49 and fall off markedly in the next two class intervals, which cover the period from 50 to 89 (Table XXXVI).[4]

TABLE XXXVI

SCORES IN SUBSTITUTION BY DIFFERENT AGE GROUPS

(From Miles, *Person J.*, 1933, 11, p. 353)

	Age Group 10-17	18-29	30-49	50-69	70-89
Number of cases	31	49	79	111	54
Substitution score	60	76	80	51	37

The experiments by Thorndike which have already been mentioned comprise only a small part of those reported in *Adult learning*.

[4] Miles interprets this form of substitution to be a measure of immediate memory. Certainly substitution tests involve immediate recall; and where the subject must write the substituted symbol in the appropriate place they are at least partially perceptual-motor. The question of classification is minor; the important thing is the change in rate of learning with changing age.

Those not described range from experiments on what he terms "sheer modifiability," such as learning to draw lines of a specified length while blindfolded, to experiments on the learning of school subjects. The character of the activities studied and the direction of the results are summarized in Table XXXVII. The decline with increasing age varies with the activity learned.

TABLE XXXVII

SUMMARY TABLE SHOWING RELATIONS BETWEEN GAINS FROM PRACTICE AND AGE

(From Thorndike, *Adult learning*, p. 103)

U = university group, including students at the Carolina College for Women
P = prisoners
H = evening high school students
S = students in secretarial schools

Group	Ability	Gain in Score: Y 20-24	Gain in Score: 25-34	Gain in Score: O 35 or over	Per Cent Which O Is of Y
U	Drawing lines	57	51	41	72
	Wrong-hand writing	28.0	27.9	22.8	81
	Substitution (transcribing words)	10.4	8.3	6.3	61
	Learning code	31.5	26.3	24.7	79
	Esperanto				64
	Learning numbers to fit nonsense syllables	21.9	18.2	14.0	64
	University studies			?	over 100
P	Substitution (1, 2, 3, etc., for a, b, c, etc.)	9.0	9.2	9.4	104
	Elementary school studies	100	100	88	88
	Addition practice	5.0	4.6	4.8	96
H	Algebra, Civics, English, etc.				87 for 30 or over
S	Typewriting				*
	Learning stenographic symbols				**

* Approximately 95 for 30 or over.
** Approximately 100 for 30 or over.

Thorndike's experiments on the learning of Esperanto by individuals varying in age from 8 to later maturity, with particular emphasis on ages 20 to 57, come closest to measuring rate of learning involving relations. Esperanto has the advantage of being a systematic body of

knowledge the fundamentals of which can be learned in a relatively short time. In the chief experiment the subjects spent 10 hours in class and 10 hours in individual study, amount learned being measured by the gains in score upon a series of four tests given before and after practice. The evidence leads him to conclude that the curve of performance in learning this systematic language by individuals who attend school through college rises from CA 8 to CA 16 and probably to CA 20 or beyond, parallels the abscissa from the age at which peak performance is reached to CA 25 or later, and then declines very slowly to CA 35 and only slightly more rapidly from there to CA 45 or later.

Qualitative Relations between Learning and Age

The greater part of the work on age-learning relations has been concerned with quantitative measurements, but qualitative changes with age which cast some light upon the quantitative changes have been observed. There is, it is generally agreed, an increase during the early years of life, in the perceived meaningfulness of verbal materials and in the attempt to learn meanings rather than words alone. This may be regarded as an indication of increasing positive transfer of training.

In their pioneer comparison of human reactions in a maze, Hicks and Carr (1912) observed that the children started out with confidence and ran rapidly, whereas the adults began "with some dignity and circumspection." Partly as a result of the frequent errors attendant on their mode of attack, the children took more time to learn. Gould and Perrin (1916) report that adults seem to frame more theories to guide their movements in a maze and to show a more systematic mode of attack. These factors and the greater habituation of the adults to routine work are thought to have been influential in determining their faster learning.

The qualitative changes with age in learning materials which involve relations are even more pronounced. With increasing age the giving of inadequate reasons for choices in a multiple-choice experiment diminishes, and reasons which take account of objective

relations, of past successes and failures, and of other more relevant features appear and become dominant (Heidbreder, 1928). The data from which this conclusion emerges provide a significant picture of a correlation between age and response to relational problems. An increasing responsiveness to problems, a positive correlation between CA and manipulation, and a negative correlation between CA and failure to respond have been noted by Matheson (1931) in preschool children confronted with problems like those used by Köhler (1925) with apes. Much earlier Lindley (1897), in the course of a study of the solution of puzzles by children of different ages and by adults, had observed in young children "the lack of circumspection," "the automatic repetitions of former movements," and the slowness to profit by their errors, which later observers have confirmed in different problem situations. With increasing CA these characteristics gradually give way to a more adequate analysis, a greater reorganization of plans at successive trials, and a more ready profiting by errors.

These and other qualitative differences between younger and older subjects are not, for the most part, peculiar to learning, but are characteristic of mental development in general. One important difference between the younger and the older lies in mode of attack. The younger are less cautious; they have other personality traits, such as less patience, which aid or hinder learning; they have different and less systematic habits of work; their perceptions of situations are less extensive; stimuli have less meaning for them; and in other ways their total mental organization differs from that of the adult and is reflected in their learning. All of these are themselves learnable characteristics, and when they determine the learning of other things, their influence may reasonably be classified as phenomena of transfer.

A Summary of the Relations between Learning and Age

The data leave no doubt that over a very wide range of activities there is an increase in measures of learning, under equal or nearly equal conditions, with increasing age during the first two decades of

life. This increase occurs both in gains from practice and in performance on the initial test or trial, which is largely a measure of transfer. It is probable that, among activities which are susceptible to incremental change from practice, there are few, if any, which will not show an increase in rate of learning with age.

The experiments on the relations between learning and age beyond early maturity are likewise in essential agreement. For some time after maximal rate of learning has been reached, it is maintained without important decrement, but eventually, and usually by the fourth decade of life, a slow decline sets in to continue for as long as measurement can be made. The fact of the slowness of the decline after the mid-twenties or early thirties is of the first importance from the standpoint both of psychological theory and of practical application.

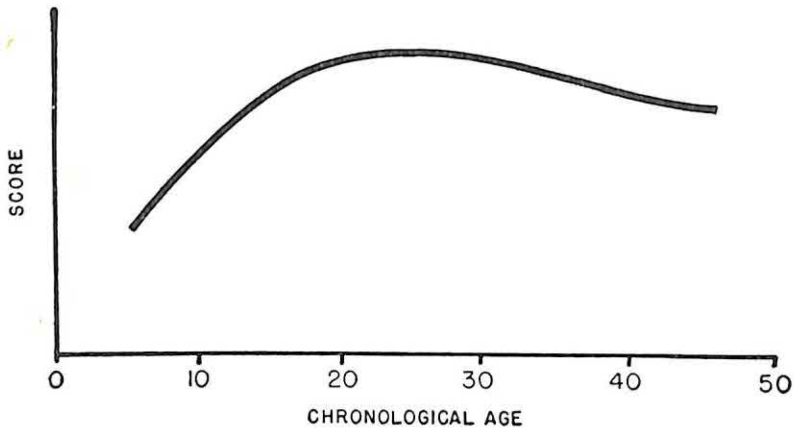

FIG. 62. A CURVE SHOWING A HYPOTHETICAL MODAL RELATION BETWEEN AGE AND MEAN PERFORMANCE ON A LARGE NUMBER OF LEARNING ACTIVITIES (From Thorndike, *Adult learning*, p. 127)

Thorndike has suggested that the curve shown in Figure 62 represents the form of the relation between rate of learning and age. The exact form of the relation is a function of a number of conditions, but the curve shown may be regarded as an estimate of the modal curve of many other curves. If we were to take the point of origin at the day of birth, the great majority of all possible curves would remain at the zero level for some time and then accelerate.

This must necessarily be true for verbal activities and for many others. The hypothetical modal curve in Figure 62 is for the ages over which Thorndike's measurements were made. It is almost certain that the changes in performance which the experiments reveal are continuous over the span of life. There is no conclusive evidence for a sudden change at puberty or for sharply insolable developmental periods in any part of the range.

Conditions of Which Relations between Learning and Age Are a Function

MATERIAL LEARNED AND METHOD OF PRESENTATION AND MEASUREMENT

There is some tendency, although its limits are unknown, for the age curves of the more ideational materials to rise more sharply, to have a less sharp deceleration, to continue to rise longer than do those for the more rote and less meaningful, and to decline less during maturity. The more complex and difficult the material, however, the greater the decline of immediate memory in later maturity (Miles, 1933b). Over the years beyond early maturity, perceptual-motor acts are learned somewhat less readily than are ideational materials.

The specific character of age-learning relations is almost certainly a function of the methods of presentation and measurement, but unfortunately the specific influence of each of the major methods has not been isloated and measured. The information in Table XXXVIII gives a rough, but important, picture of the unanalyzed influence of methods and materials. Gilbert (1941) matched 174 individuals between CA 60 and 69 with 174 between CA 20 and 29 on vocabulary score, which is a good single measure of intellectual level, and tested each one with the materials listed in the table. The table gives the percentages of loss for the entire group and for the 40 in the older group who scored highest on the vocabulary test. The deficit shown by the highest 40 in intellectual level was determined by comparing them with the corresponding 40 in the younger group.

The smallest deficit occurs in the immediate recall of brief new

material, the greatest in the learning with brief exposure of more complex new material, especially when there is an opportunity for interference between the new associations and ones previously formed by the subject. It is probable that such details of method as time of exposure or frequency of repetition are also effective conditions. The bright group show a smaller deficit than do the total group, but their relations between material and amount of loss are closely similar to those of the total group. The variable of the material learned leads naturally to that of transfer, because the two are so closely interrelated.

TABLE XXXVIII

PER CENT OF DEFICIT OF PERSONS AGED 60 TO 69 RELATIVE TO PERFORMANCE OF MATCHED SUBJECTS AGED 20 TO 29

(From J. G. Gilbert, *J. abnorm. soc. Psychol.*, 1941, 36, pp. 79 and 83)

Test Material	Per Cent Loss Total Older Group	(Younger–Older)/Younger The 40 'Brightest' Older Group
Visual memory span for digits	8.5	5.2
Auditory memory span for digits	11.8	8.4
Reversed digit span	21.2	18.8
Sentence repetitions	21.3	10.3
Knox cubes (nonverbal repetition)	26.2	14.4
Retention of paragraph	39.7	18.6
Immediate memory of paragraph	41.8	20.6
Memory for designs	45.9	24.6
Retention of paired associates	54.6	30.3
Paired associates	58.7	36.3
Retention of Turkish-English vocabulary	60.4	32.0

AMOUNT AND SIGN OF TRANSFER

Initial performance of a new task is a function of a number of variables. One of the most important of these, certainly, is transfer of training from prior learning. Within certain limits, it is probable that the amount of this transfer increases with age. Certainly, the stage is set for this conclusion by the fact that a subject's

learned organization grows richer and more complex over the years from birth to early maturity. What effect this previous learning will have upon the initial performance of a novel task will depend upon a great many variables including the amount of previous learning and the appropriateness of the previously learned responses in the new problem situation. (See Chapter IX.)

There is fragmentary evidence that directly measured amount of *positive* transfer increases during the early years of life, other things being equal. Wieg (1932) finds it to be true in bilateral transfer with children aged 66 to 88 months, and Roberts (1932) reports it in problem-solving at CA 3, 4, and 5, but not at CA 2. Conversely, Thorndike (1928) concludes that older subjects are more susceptible to *negative* transfer.[5] If this conclusion is valid, it is of great importance for an understanding of the decline in rate of learning which often occurs during late maturity.

The results obtained by Ruch (1934) bear both upon this last conclusion and upon the character of the material learned as a condition of age-learning curves. His learning materials were chosen for their relation to the prior experience of the subjects and were: (a) direct-vision rotor (pursuitmeter), requiring the partial use of already acquired responses; (b) mirror-vision rotor, requiring the breaking of old direct-vision responses and the formation of new connections; (c) paired associates (logically connected words), involving the use of old associations; (d) nonsense equations, as $E + Z = G$, demanding the formation of new verbal associations and measuring what Thorndike has called "sheer modifiability"; and (e) false products, as $3 \times 5 = 25$, the learning of which involves associative interference between the old connection and the new. The subjects were three groups of 40 each, distributed over the CA intervals 12–17, 34–59, and 60–82.

[5] Certain results of Willoughby's (1929, 1930) give inferential support to the conclusion that susceptibility to negative transfer increases over the years of maturity. Upon analysis of the serial positions of the recalled items of a digit-symbol code he finds that the older subjects give a serial position curve with a more pronounced dip than that which appears in the curve for younger subjects. (children of the older ones). If we interpret the inferiority of the intermediate serial positions to be a function of interference, we must conclude that negative transfer is greater for these older subjects.

The mirror-vision rotor yields a greater deficit for the old group than for the young, and the amount of deficit on the verbal materials increases in the order: paired associates, nonsense equations, and interference material. Comparison of the oldest group with the middle-aged gives the same order of deficit, though with less statistical significance. Ruch interprets his results to mean that the older subjects encountered their greatest difficulty in learning activities which demanded a reorganization of already established responses and their least difficulty in learning those activities requiring the least reorganization. This interpretation makes the material learned and the amount and sign of transfer to it from the subject's existent repertoire of response a fundamental condition of the relations between learning and age over the mature life span.

Just why it should be that early in life prior learning generally has a positive transfer effect, while later in life it has a negative effect, is not clear. It may be, of course, that factors of maturation and degeneration may account for this shift. On the other hand, it is entirely possible that, over a long period of time, shifts in the type of learning required make the previously learned responses of the older subjects less appropriate. It is also possible that, in the older subjects, the greater amount of previous practice renders further modification of the practiced habits more difficult.

TRANSFER FROM PRACTICE AT FORMAL LEARNING

Furthermore, it is an often-raised question whether the obtained relations between age and rate of learning, particularly during later life, may be a function of the extent to which the subjects measured have kept in practice at formal learning. At least part of the influence of practice at learning certain activities on rate of learning other and new ones is a phase of transfer. The question asked is important, because so often older persons do very little formal learning, and their deficits under experimental measurement may be partly a function of their lack of recent practice.

The passage of time without practice at formal learning may be accompanied by forgetting, so that as individuals grow older and,

being no longer forced by the demands of school or the requirements of attaining competence in a trade or profession to practice specific activities, cease to indulge in systematic practice, they lose a considerable amount of potentially transferable habits of work. They probably lose also a large amount of information which might transfer to some of the materials on which they are tested in experiments on age and learning. The decline which the experiments have measured over the later years of life may be, then, a function of forgetting. We have seen in Chapter X that forgetting itself is, in part, a phenomenon of negative transfer, which accounts for its having been mentioned here under the heading of transfer.

The conclusion reached by Thorndike in *Adult learning* (p. 146) that continued practice at learning is "probably a partial preventive or cure for adult inability to learn" is supported by some results of Sorenson's (1930). Miles (1935) has organized evidence from the Stanford Later Maturity Studies to show that training and practice account for a large part of the gains which appear from year to year and that practice, if steadily kept up, may serve to maintain one's mental status into the later years of life.

This conclusion is reasonable on the grounds of what is known about transfer. Practice at learning keeps active one's methods of study, yields an increasing body of potentially transferable knowledge, and may indirectly increase motivation. The operation of continued practice may be a powerful determiner of the increasing scores with age up to early maturity, and its absence may be a determiner of the decline during later maturity.

MENTAL AGE

As individuals increase in chronological age from infancy to early maturity they may also increase in mental age as measured by intelligence tests. CA and MA, although correlated, are separate variables, and each is correlated with measures of rate of learning. It is desirable to know the extent to which the relations between CA and learning are a function of the correlation of each with MA.

In a study of multiple-choice learning by 40 children of CA 3:6 to

7:10 and of MA 4:0 to 6:4. Roberts (1933) finds a correlation of $-0.40 \pm .089$ between CA and number of trials required to learn the first problem and of $-0.47 \pm .083$ between MA and number of trials. When MA is partialed out, the coefficient of -0.40 becomes $-0.17 \pm .103$, but when CA is partialed out, the coefficient of -0.47 is reduced only to $-0.30 \pm .097$. Roberts also held MA constant experimentally by selecting the 15 children who fell within a range of one year MA (MA 4:0 to 4:11) and by computing for these subjects the rank-difference correlation between CA and trials. The result was $-0.14 \pm .149$. When a similar equation was made in terms of CA and the correlation found between MA and trials, the result was $-0.41 \pm .128$.

These data show a higher correlation between MA and trials than between CA and trials, and a considerable decrease in the size of the coefficient when MA is held constant either by statistical or by experimental means. This decrease is larger than the one which occurs when CA is held constant. The number of cases is small, and the age range covered is narrow, but the data suggest that MA is here one important determiner of the correlation between CA and trials to learn the solution of the problem employed.[6]

When MA is partialed out, the correlation between CA and poetry scores in the experiment by Stroud and Maul (1933), already cited, drops from 0.61 to 0.03, and that between CA and nonsense-syllable scores drops from 0.49 to -0.02. Partialing out CA reduces the MA-learning correlations somewhat, but much less than does the partialing out of MA. Fractionation of the subjects into groups having constant MA and variable CA within each group fails to reveal any clear increase in learning scores with increasing CA.

These data, which are representative of those available, suggest that over at least a considerable range of activities MA is an important condition of the relation between CA and learning. The influence of MA is usually found to be greater over the age range

[6] Correlations of similar import have been published by Kirkwood (1926), whose subjects over the age range 4:0 to 5:6 learned certain simple associations; by Conrad and Jones (1929), who took recall of a motion picture by subjects over the range CA 10 to 16; and by Dietze (1931), who studied immediate memory for factual material over a wider band of ages.

from infancy to early maturity than over the range from early maturity to old age, although Gilbert found, as we have seen, that amount of deficit in old age is less for the more intelligent. The mental test operations which give a mental age score measure, in part, transfer from both scholastic and nonscholastic training. It would have been reasonable, therefore, to classify the data just discussed as a special case of transfer. They have been treated separately because the operations of measurement are different, but the implications of the two sets of data are the same. The relations between learning and CA are to a very large extent statements of amount of transfer.

MOTIVATION AND PERSONALITY CHARACTERISTICS

Motivation is an important variable in studies of relations between learning and age, because motivation, which is a determiner of learning, probably varies with CA. Very young children can scarcely be motivated to work at the more formal laboratory activities at all, and special rewards or special instructions, not suitable for older persons, must be used. Little is known about the specific changes in motivation during the conventional school years, but there is little doubt that such changes do occur. There is, however, observational evidence for a frequent decline in motivation to learn after adulthood has been reached. The adult may cease to want to learn, as Thorndike has suggested, because he feels that he has learned all that he needs to know and because he is then occupied by the practice of his trade or business or profession. This may become increasingly true with advancing age, particularly when he is asked to learn the materials which experiments employ. The curves of relation between learning and age which our measurements give us are, thus, unanalyzed composites of performance due to motivation and the manifold other conditions which determine learning.

Closely allied to differences in motivation are differences in what may be classed as traits of personality. We have seen that children are often less cautious, less systematic, and less reflective than adults. These traits and doubtless many others influence behavior during

practice. As an individual grows older from early adulthood to old age he may, and often does, become more sensitive to ridicule, less willing to attempt novel activities, less tolerant of practice at activities which lie outside his more immediate interests, vocation, or proficiency, and more possessed of other personality traits which retard practice. The deficit found in rate of learning may be to a considerable extent a result of these characteristics.

Hypotheses To Account for the Relations between Rate of Learning and Age

The increase in rate of learning with age over the years to early maturity, its relative constancy during the next decade, and its slow decline thereafter appear over a very wide range of conditions and are among the more general facts of the psychology of learning. An hypothesis to account for these facts needs to apply to a wide range of years and, if possible, to the entire span of life. Certain hypotheses which are designed to account for the changes over the whole course of the age-learning curve will be presented.

THE MATURATION-DEGENERATION HYPOTHESIS

(a) *Maturation.* This hypothesis is formulated on the analogy of physical growth and transfers the pattern of its explanation to the explanation of age-learning relations. A normal human being develops physically from infancy to maturity under the influence of organic, and probably of inherited, conditions. Stimuli from the environment are necessary and may exert an important influence on development, but under normal conditions this influence is relatively small, and, in any case, only acts to modify the operation of endogenous conditions. The inference of the maturation hypothesis of the age-learning relations is that the increase with age in rate of learning is another phase of organic development—that is, a maturation phenomenon.[7]

[7] It should be understood that the equivocal nature-nurture issue is not raised here. The inquiry concerns only the evidence on inner growth in the organic correlates of learning insofar as this growth is a fundamental condition

There have been many definitions of maturation, and it has often been vaguely used. No evaluation of these many definitions will be made, but the term maturation will be used to include *any change with age in the condition of learning which depends primarily upon organic growth factors rather than upon prior practice or experience.* A maturation hypothesis of the increase in rate of learning with age may have any one or any combination of three specific meanings. (1) There may be a maturation of the sensory and motor mechanisms required for learning so that, as the individual grows older, he can learn a given material or activity more readily than he could at an earlier age because he has stronger, larger, faster, or better-coordinated mechanisms. (2) There may be a maturation of plasticity or modifiability, which, aside from increases in sensory and motor functions, facilitates learning. This seems to be the meaning which many writers have attached to it in the setting of the present problem. (3) There may be a maturation of the specific functional organization necessary for performance, an organization like that underlying the more usually accepted forms of native behavior, so that any given act either has a progressively higher initial status at successive ages or shows progressively greater gains from practice, or both.

There have been no experiments designed to test directly the contribution of maturation to the relations between rate of learning and age over a large band of ages, although there have been experiments which bear on the problem. The experimental method and the general outcome of the available experiments will be discussed. The methodological paradigm which the experiments follow is identical with one for the measurement of transfer of training (Chap. IX), with the single difference that the initial and final tests are made upon the performance which has been trained rather than upon a different one. Two groups of subjects are equated in terms of initial status in the activity to be used, in terms of CA, and often in MA and other related measures. One is given no specific practice

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of age-learning relations. On general principles of the congenital basis of mental development, see Carr (1925). Actually, the question of an explanation of age-learning relations has received little direct examination.

on the activity beyond that provided by the test of initial status, but is tested a second time after the lapse of an interval. The other is given further practice, usually on the same activity as the one initially tested, and is then tested again after an interval which is the same as that for the other group. The procedure may be summarized as follows:[8]

Group A:	Initial test	Period of no practice	Final test
Group B:	Initial test	Period of continued practice	Final test

If the two groups are equal at the final test, or if Group A shows a significant gain, regardless of whether Group B shows the same or greater, most experimenters conclude that maturation has been effective.

The available experimental results[9] agree in finding only a small superiority of practiced subjects over unpracticed ones in the case of simple activities. They agree also in finding at least small increments from initial to final tests on the part of the unpracticed groups. These facts may be, however, a function of at least three conditions, not all of which are always taken into account: (1) the practice afforded by the tests; (2) maturation; and (3) transfer from parallel practice on related activities in daily life. These three have been present in unknown proportions, and nothing exact can be said about their relative effects.

The evidence for maturation as a condition of age-learning rela-

[8] The theory behind the experimental methods is contained in the following statements from Thorndike (1928): "If by a miracle a child of ten could be kept alive and well, but in a dreamless sleep, for a year and then waked up to resume its ordinary life, it would not thereafter be a year behind in ability to learn. A year brings growth from within as well as training from without, and the loss of the latter need not involve, in theory, at least, the loss of the former." The period of freedom from practice under the control condition is the experimentalist's substitute for the miracle of the dreamless sleep.

[9] For interpretations of maturation and for experimental results on the general problem of maturation and learning the following references are important. Discussions of the meaning of maturation and of relevant evidence will be found in Witty and Lehman (1933), Stone (1934), and Carmichael (1936). Carmichael's presidential address (1941) contains background material. Experimental work is reported by Gesell and Thompson (1929), Strayer (1930), Hicks (1931), J. R. Hilgard (1933), Jersild (1932), Mattson (1933), and Dennis (1935).

tions rests upon evidence of this kind. The precise contribution of maturation has not been isolated, although it is reasonable to suppose that it is present in one meaning of the term. Certainly, a child cannot climb stairs, name colors, run a maze, or do anything else until his organism has the strength, speed, or other physical properties requisite for the activity, and growth in these respects may be expected to be an essential condition of learning. If the maturation hypothesis means only this, it hardly needs experimental evidence to support it, although the experimental facts may serve to emphasize its importance in a given case.

The proponents of the hypothesis have seemed to mean more than this, though they have seldom said so explicitly. They have seemed to mean a maturation either of specific organization of the same kind as that brought about by practice or else an increase of plasticity or modifiability. There is, however, nothing in the data which requires such an hypothesis. The difficulty here stems from our lack of knowledge concerning the physiological changes which underlie the learning process. If these changes were identifiable, and if the structures which mediate them were more adequately known, an evaluation of this form of the maturation hypothesis would be possible. Unquestionably, the individual increases in plasticity or modifiability sometime between the moment of fertilization and the attainment of his maturity. Whether this increase in plasticity precedes the maturation of sensory and effector organs, or whether it lags behind this maturation, is not known.

(b) *Degeneration.* The interpretation of the decline in rate of learning which appears in maturity and later maturity has received relatively little attention. Explanation of this decline by an assumed inner degeneration is, however, a corollary of the explanation of the earlier rise in terms of maturation. This degeneration might affect any or all of the three sensory and motor mechanisms, specific organization, and plasticity. It will be discussed here with reference to the normal individual, thus excluding those suffering injury or disease.

In *Adult learning,* Thorndike has given this theory its major consideration. He has examined the data of other investigators upon

changes with age in strength, speed of movement, and sensory discriminations, and has found that, while there is a decline from the early twenties to later maturity, it is very slow and very small in absolute amount. It is possible, moreover, that this decline might also be somewhat influenced by experience, although experience could hardly account for more than a part of it.

Thorndike's own data upon what he calls "sheer modifiability" show a greater decline with age than do the data upon some activities to which past training can transfer more readily. Drawing lines with one's eyes closed, incidental learning of a code, connecting numbers and nonsense syllables, are examples of sheer modifiability, and school studies are cases of activities which may benefit more by transfer. He points out, however, that the data are not consistent in this respect. It is doubtful if we can make a valid distinction among activities regarding influence from transfer in the absence of more specific experimental measurements. Moreover, as Thorndike suggests, the older subjects may have been more self-conscious about attacking the novel problems than about studying school subjects and may have differed from the younger in other ways which had developed through experience. Thorndike is led by these and similar considerations to the conclusion that inner degeneration is not the sole condition of the decline, but he seems to believe that it is one condition.

The work of Ruch (1934), already mentioned, departed from the biological concept of senescence as a "condition of lowered plasticity of tissue." On the assumption that learning involves tissue changes, later maturity should bring a differential set of changes in rate of learning. Old subjects should have greater difficulty in learning activities which demand relatively greater reorganization of already learned connections than in learning activities which demand less reorganization. Ruch found that his experimental tasks yielded the relative deficits which would be predicted on the basis of the assumption stated.

Ruch's interpretation is possible and plausible, but there is also the possible interpretation that the deficit of the aged in learning associations which interfere with those already in their repertoire

is a function of more strictly psychological conditions. Among these may be a more effective operation of the conditions determining negative transfer, a dislike of change, and other analogous personality characteristics. The data for a confident conclusion are not at hand, but what data there are suggest that, aside from a decline in sensory and motor mechanisms upon which learning depends, there is on the average only a relatively slight degeneration of neural organization or of plasticity.[10]

THE HYPOTHESIS OF CHANGING CONDITIONS OF LEARNING

It may be that the major features of the obtained relations between age and learning are partly accounted for by progressively changing psychological conditions. The conditions which now seem to be the most important have already been discussed and can be briefly dealt with here. Chief among them are transfer, motivation, and personality traits. It is questionable that they account for all of the changes with age, but there is no doubt that they act together with whatever maturation and degeneration of organic mechanisms may also be influential.

The possibilities of positive transfer to most of the activities and materials which an individual is likely to be called upon to learn increase markedly during the years from infancy to early maturity and may reasonably be supposed to outweigh negative transfer effects by a considerable margin. It is a reasonable inference that they remain high during the next decade or so, when the balance between positive and negative transfer in many cases shifts more and more toward the side of negative transfer. These inferences are supported by the evidence already presented.

[10] There are, of course, a number of mental disorders of the aged which involve disturbances of learning and retention and which are accompanied by observable changes in the nervous system. Alzheimer's disease, Pick's disease, psychosis with cerebral arteriosclerosis, and senile dementia all tend to occur late in life, all may result in lowered capacity to learn and to retain, and all involve characteristic pathological brain changes. That these brain changes are responsible for the lowered capacity to learn appears to be a reasonable assumption. That non-psychotic individuals suffer a lowered learning capacity at advancing ages due to analogous, but less severe, organic changes is a plausible hypothesis, but one which goes beyond our present knowledge.

The transfer in question may be in terms both of habits of work and of more specific behavior. During the years from infancy to early maturity, habits of work become on the average increasingly effective, and during the years of later maturity they may narrow down to the settled requirements of life, being adequate for these requirements but less effective when faced with new materials. Over the years to early maturity, the individual is also acquiring an increasing repertoire of potentially transferable training, which may aid the learning of new things, while during his later years his retained repertoire interferes with the acquisition of the new.

A parallel progressive change may occur in motivating conditions and personality traits. In his earlier years, an individual is fascinated by the new and is not as likely to be kept from attacking it by dignity, fear of comment, or other personality traits which may characterize him in later maturity. Likewise, in the later years, he may have acquired a more narrowly stabilized mode of living in which he is not motivated to learn new things.

It is reasonable that changes in ease of learning should be a function of such changes as these in the conditions of learning, but the hypothesis needs systematic testing throughout the life span. It may be regarded as paralleling that of maturation-degeneration, and we now proceed to a brief statement of the way in which both hypotheses seem to apply.

APPLICATION OF BOTH HYPOTHESES TO THE DATA

The complex phenomena of human behavior are determined by multiple conditions, and it seems probable that the relations between age and ease of learning need at least the two hypotheses mentioned to explain them. The organism provides the framework of mechanism within which learning occurs and by which learning's bounds are set. Maturation and degeneration probably change this framework in many now unknown ways which subtly affect rate of learning. Within the bounds set by the organism, the psychological conditions of learning change and have their influence.

The two sets of conditions operate together, having relative weights which are a function of the material learned and of many other conditions.

Thus, children may master motor skills more slowly than more mature learners because of a less well-developed motor equipment, while the aged may learn more slowly because of sensory, connecting, or motor mechanisms which are less effective than they were at maturity. Children may learn ideational materials more slowly than the mature because of less retained experience which will transfer positively to the new, and perhaps because of less effective motivation to learn such materials. The aged may learn similar things more slowly than the mature because of negative transfer, or because they are less highly motivated to learn them. In many ways, the two sets of conditions may operate together and, to extents now unknown, may be functions of each other.

SEX DIFFERENCES IN LEARNING

The subjects of all experimentation are dichotomized by the biological fact of sex, which shares with age the status of an inescapable variable, and the question whether this biological fact implies a corresponding difference in mental traits has occupied experimenters ever since the importance of individual differences was recognized. An answer to it will have both scientific importance and significance for social, educational, political, industrial, and other such practical concerns.

The number of comparisons of the learning of the sexes is very large, and the results have signally failed to show any constancy of direction. Instead, they group around three modes: (a) those which show a difference in favor of males; (b) those which show a difference in favor of females; and (c) those which show no significant difference between the sexes. There are several facts which must be considered in an interpretation of these divergent conclusions. The size of the obtained differences is relatively small in most cases, and the overlapping of the distributions is great. In some cases

there is little doubt that the differences are a function of sampling errors,[11] but in others, where the direction of difference is a constant over a wide range of CA and where the samplings are large, sampling errors cannot be invoked in the absence of specific reason for believing that they exist. We are left with at least a few significant but small sex differences in both directions, as well as with a relatively large amount of data which reveal no significant differences.[12]

The data show no clear or uniform tendency for sex differences to vary CA and give no clear support to the assumption, sometimes made explicitly by writers on sex differences, that girls should excel boys of equal CA during the years up to and including adolescence because of the earlier maturing of girls, but that after adolescence the difference should disappear. The fact that there are no uniform and regularly appearing sex differences tells against this assumption. The fact, however, that some experiments have found differences between the sexes to become less toward adolescence or even to reverse their direction has been interpreted in support of it (see Figs. 54 and 55). Many experiments, on the contrary, have found no tendency for sex differences in learning to diminish or reverse at adolescence, and so long as the phenomenon is far from being general it can scarcely be attributed to differences in rate of maturization.

[11] Many of the reported differences between rates of learning of the sexes have been obtained from samplings too small and unrepresentative to be sufficient. Consistency of results at each age over a band of ages would go far to compensate for the smallness of the samplings at each age if all samplings were equally representative. Such an equality is difficult to obtain from a school population, which is the one almost universally used, because of the different operation of social and economic factors upon the sexes. These may affect the age at which the child is first sent to school, the school to which he or she is sent, his regularity of attendance, and the age of leaving. The last is probably the most systematic, as well as the most obvious, in its action. This factor, as well as the others, will operate differently in different communities and in different states.

[12] By no means all of the experiments have been strictly comparable. They have measured memory span, immediate memory of lengths beyond the span, time, trials, or errors to attain a criterion, and amount learned in a constant time. To analyze out the results of each different measure and condition would be of little value here, though it is worth mentioning that the diversity and incomparability exist.

Conditions of Which Sex Differences in Learning Are a Function

In spite of the large number of reported comparisons of the sexes, there have been few systematic investigations of the conditions which determine obtained differences in rate of learning. Instead, experimenters have seemed to be searching for fixed and constant differences, presumably differences determined by innate conditions. None of the latter has been found, save for those based on obvious physiological differences, and the reasonable thing seems to be to look for possible determiners of the obtained results, which will permit us to understand their variable direction and amount. Where the samplings have been representative and the differences significant, the results may have been a function of many of the experimental conditions, especially of method of measurement. There are two conditions, however, on which there is sufficient evidence to warrant a conclusion, and the discussion will be confined to them.

AMOUNT OF PRACTICE

The great majority of the data on sex differences in learning are measures either of initial performance or of a very few practice trials. A few experiments, however, have measured the learning of the sexes under continued practice. McGinnis (1929), for example, gave her subjects, aged 3, 4, and 5, fifty trials at a relatively simple maze problem and found that the early differences in favor of the boys diminished with practice. This result is corroborated by a small number of other studies. To the extent that practice diminishes sex differences, they must be ascribed to conditions of stimulation and activity rather than to fundamental biological conditions.

THE CHARACTER OF THE MATERIAL OR ACTIVITY LEARNED

A number of experiments reveal that the direction and amount of sex differences in learning are a function of the material being learned. Pyle (1925b) has attacked the problem by asking 196 boys and 288 girls, all in high school and about equally distributed over

ages 14 to 17, to learn four sections of prose of differing content. Materials A and B were technical and dealt with physical, chemical, and mechanical processes; Material C was about furs and the animals from which they come; and Material D was a set of geographical data about a fabled island. The boys excelled slightly with Materials A and B, the scores of the sexes being 11.50 and 10.08, 8.33 and 7.70, respectively; the girls excelled with C, the scores being 23.03 and 26.00; and there was a very small difference (0.48) in favor of the boys with Material D. These results may be interpreted to mean that subjects of each sex learned better the material which most interested them and which was most relevant to their prior experience, while material having no differential appeal was acquired with equal readiness by the two sexes.

The influence of the material learned has been studied in another way by Boynton (1931), who used 20 colored pictures designed to appeal to girls and 20 designed to appeal to boys, a list of 20 names of things pertaining to girls and of 20 pertaining to boys, and lists of syllables and other materials with no apparent difference in appeal to the sexes. The girls earned the higher scores on the material designed to appeal to them, but where it was not so designed this superiority disappeared.

These two experiments have compared total scores on a given material and have taken no account of sex differences in the learning of component parts. Conrad and Jones (1931) have examined the individual questions in their tests of the immediate recall of motion pictures and have found reliable sex differences in response to individual items. The members of each sex excel on questions relevant to their own peculiar activities and training. Earlier, Bassett (1929) had found among pupils in Grades VI A, VII A, and VII B only a slight gross difference in favor of the boys in delayed recall of history, but differential responses to individual items. Boys were superior in knowledge about war and geographical places; girls were superior in recall of material dealing with domestic conditions and home life.

These results reinforce the conclusion from less analyzed material that obtained sex differences are a function of the material

learned. When one considers the wide differences which exist between the sexes in physical and social training, in daily activity, and in the manifold aspects of daily life, it is reasonable to expect differences in the appeal which any material will make and the prior training which can be brought to bear upon it. These differences reduce to differences in motivation and in the possibilities of positive transfer.

Although it has been the custom to emphasize sex differences, it is even more important to realize the extent to which the sexes are similar in the learning of a given activity. Where gross sex differences are as small as those usually reported, the similarities must far outweigh the differences. Conrad and Jones (1931) correlated the percentage of correct answers given by males with the percentage given by females for all of the questions on a given material and obtained coefficients ranging from 0.78 to 0.93, thus demonstrating a relatively high similarity between the sexes in spite of the differentiation shown by individual items. Further investigation revealed only a small relationship between adolescent and adult sex differences, which implies that sex differences themselves are subject to differentiation under the changing conditions of experience.[13]

SUMMARY AND INTERPRETATION

Sex differences in rate of learning are small and do not consistently favor one sex or the other. Which sex will prove superior in a given experiment is a function of amount of specific practice involved in the measurements and of the character of the activity or material. Both conditions are a matter of training, the first of training at the specific test activity, the second of training at related things. There are doubtless other conditions, also, about which less is now known.

[13] In certain experiments on perceptual-motor learning—for example, Irion (1949)—sex differences have been found. Although this is fairly characteristic in the case of certain learning activities, differential cultural backgrounds may be responsible. In cases where strength or size plays an important role, innate sex differences appear, as may be determined by a glance at comparable athletic records for men and women.

The question at once arises whether differences in the rates of learning of the sexes, when obtained in significant amounts, are wholly attributable to conditions of present stimulation and prior training or whether native differences must also be invoked. There is no crucial positive evidence that native differences do not play a part, but the evidence that differences in stimulation and training can account for the obtained differences in many cases is so cogent that there seems little need to call on native conditions, except insofar as they operate to favor particular classes of prior training and interest.

LEARNING AS A FUNCTION OF TEST INTELLIGENCE

Single learning experiments deal with a narrower sample of human behavior than do intelligence tests. The experiments deal also with changes during practice which are not known to be diagnostic of any general characteristic, while intelligence tests do not directly measure changes during practice and are designed to be diagnostic of ability to succeed in school and in other socially valued activities. Without entering upon the theoretical issues raised by intelligence testing, we may treat test scores as independent variables and learning records as dependent. We have already discussed the relation between rate of learning and CA; here we inquire about the correlation between the rate of learning of a particular material and some standard measure of intelligence, when the influence of CA is as nearly as possible a constant.

It is clear, even on casual analysis, that intelligence test scores themselves are not independent of learning. Instead, the tests make certain assumptions which directly involve learning. They assume, first of all, that all of the persons tested have had an *equal opportunity* in everyday living or in school to learn the materials which the test uses, such as words and numbers. They also make one or both of two further assumptions. The first is that all subjects actually have learned the materials used, but that differences which appear in test scores are differences in the use of these simple, familiar materials in formulating new relationships and in solving problems. This ability to use what has already been learned is, in part, an il-

lustration of transfer of training, although it has not been customary to speak of intelligence tests as being tests of transfer. One reason for this is that, even though the assumption of *equal opportunity* may be valid, it would be unwise to assume that the subjects had had *equal prior practice*.

The second assumption is that, although all have had an equal opportunity to become acquainted with the materials of the test, all have not learned them to the same degree, the measured differences in this respect being reflected in the measures of intelligence. The first assumption is illustrated by the well-known analogies test; the second by information tests, such as the one in Army Alpha, or by simple learning tests, such as those in the Stanford-Binet.

Intelligence tests are, thus, measures of learning in which direct learning and transfer are mingled in a variety of ways. The experimental question here is to what extent intelligence test scores correlate with measures of learning obtained under experimental conditions. For the sake of brevity, the test scores will be called "intelligence" and the specific measures of learning will be called "learning," but these words should be understood to refer directly to the specific records under discussion.

Questions of Methodology

There are several ways of measuring intelligence and of expressing the scores. There are individual and group tests, verbal and non-verbal tests, and performance upon them may be expressed as raw scores or in terms of such derived measures as mental age (MA), intelligence quotient (IQ), percentile scores, and sigma scores. There are also numerous ways of measuring learning experimentally, and a correlation between intelligence and learning needs to be accompanied by a statement of the specific variables which are placed in correlation.

Chronological age must also be controlled. When, without further analysis, CA is placed in correlation with rate of learning, CA includes MA plus other factors. CA and MA are correlated, but to

the extent that CA includes other variables than MA its influence must be held constant if we wish to discover the relations between MA and rate of learning. CA can be held constant experimentally by using subjects of the same age. Adults of not too widely separated ages may be treated as if they were of the same CA without important error. CA may also be partialed out statistically, but experimental control is to be preferred to statistical control.

The IQ is an index number obtained by dividing CA into MA. MA refers to the level of mental maturity as determined by the operations of testing, while IQ reflects the rate of mental development. On the average, MA increases with CA up to some point which varies with the individual, while IQ, under many conditions of life and training, remains roughly a constant. These statements follow from the ways in which tests have been constructed and standardized. It also follows that when the CA of a group is a constant, MA and IQ provide the same information about the individual members.

Total score on group tests and raw scores expressed as sigma values have the same general meaning that MA has. The tests are made so that these scores will increase with CA in a series of adequately sampled age groups. Each represents a level of performance, and each is a function of CA.

The measurement of learning may be of gains from practice, of initial performance, or of a composite of the two. The use of initial performance as a measure of learning requires discussion. In connection with learning tasks which are identical with, or highly similar to, intelligence tests or sub-tests, initial score may correlate highly with intelligence. Take, for example, the digit-symbol substitution task. This may be used as a learning task. It is also used as a sub-test in the Wechsler (1944) scale. If the trials in the learning experiment coincide in length and method of administration with the procedures used in testing intelligence, we would expect scores on the initial learning trial to coincide with scores obtained on the intelligence sub-test. If we allow this initial score to enter the determination of both intelligence and learning, we have spuriously raised the correlation between these two variables. Thus, gain

scores rather than initial scores or a composite of initial and gain scores should be placed in correlation with intelligence scores.

The representativeness of the sampling is always important, but where coefficients of correlation are computed the heterogeneity of the sampling also becomes important, because, other things being equal, the size of a coefficient increases as the range of the scores placed in correlation. Most of the studies of the relations between learning and intelligence have employed relatively narrow ranges, and this fact must be taken into account when interpreting the results. The range of both intelligence and of learning in a group of college sophomores is short compared with that in a sample of the total population.[14] That of a group of pupils in the elementary or secondary schools is much wider, but it is still less than that of the total population. It is a justifiable inference that most or all of the published coefficients of correlation between learning and intelligence would have been much higher had the sampling included subjects at every point on the intelligence scale from the lowest intelligence level at which the task employed could be discriminably learned to the highest measurable level.

When we deal with coefficients of correlation, the problem of the reliability of the measurements—the degree to which they consistently measure whatever they measure—becomes acute, because then we are comparing individuals, not means. Only a few of the published coefficients have been corrected for unreliability of measurement. The use of more reliable measures and correction for attenuation would yield higher coefficients. When we add the use of measures uncorrected for attenuation to the use of relatively short ranges

[14] Just as it is difficult to construct learning situations which apply equally well to widely different age groups, so it is difficult to find a situation which will be useful in the study of the learning of groups of widely different intelligence levels. A number of situations for the study of learning among aments have been developed. McPherson (1948) reviews a number of such studies. Other representative papers include those of Rethlingshafer (1941a, b, c) on the Zeigarnik phenomenon; Gardner (1945) comparing the learning of aments, horses, cows, and sheep; and Fuller (1949) on the conditioning of a vegetative human organism. Studies of the achievement of subjects of high intelligence include those of Hollingworth and Cobb (1928), Terman (1940), and Terman and Oden (1947).

of score, the remarkable thing is that so many of the obtained coefficients have been as high as they have been.

Under the head of method comes also the method of measurement. The intelligence test used and the specific measures of learning are conditions with which the size of the correlations has been found to vary. So, also, are many aspects of the experimental method used, some of which will be implied in the following discussion of the influence of the activity learned.

Verbal Materials

It would be pointless to reproduce a large number of individual coefficients. It will be sufficient to summarize briefly the conclusions from them. The first general conclusion which emerges from a study of the numerous measures which have been placed in correlation is that the relation is positive almost without exception. The size of the coefficients varies from close to zero to as high as 0.60 or 0.70. Incidental learning and memory span yield coefficients which are relatively low; meaningful material gives coefficients which are often relatively high. When gain scores are used, however, the relationship between intelligence and improvement is disappointingly low. Woodrow's (1946) authoritative paper reviews a number of instances where intelligence and learning are unrelated and demonstrates how it is possible for intelligence to be positively correlated with achievement at every stage of practice and yet uncorrelated with the rate of gain which the practice produces. Part of the lack of correlation between learning and intelligence, doubtless results from a restriction of sampling, and part evidently results from the lack of any general factor of learning ability which determines rate of improvement in a variety of learning situations (Woodrow, 1940).[15]

[15] Some of the evidence concerning this topic will be found in the papers mentioned in this note. Whipple's *Manual* (1921) reviews many of the early results on relations between memory span and estimated intelligence, or memory span and school standing. Garrett's (1928) study gives correlations between the Thorndike Examination and a number of measures of learning in a group of 158 college men. Other papers include: simple associations by

Perpetual-Motor Activities and Relational Problems

A majority of the correlations computed between measures of intelligence and of perceptual-motor learning are positive in meaning. They vary in size from zero to 0.70 or slightly higher, but there are many more below 0.50 than above it.[16] These statements hold for measures of learning which are unanalyzed composites of initial performance and gain and for measures of initial performance alone. In some cases, as in the data reported by De Weerdt (1927), the correlations with gain are of the same order as those with initial performance, but there are only a few experiments which have attempted to isolate the two. Comparisons of selected groups at clearly separated points on the scale of MA or of IQ have uniformly shown a group difference in favor of the more intelligent.[17]

Measures of rational learning, of problem-solving which involves abstract relations, of reasoning, and of inference have been placed in correlation with intelligence test scores, with resulting coefficients of correlation which range from exceedingly low to as high as 0.70 or 0.80.

Multiple Correlations between Intelligence and Measures of Learning

Most studies of relations between learning and intelligence correlate the scores on an intelligence test with some single measure of learning. A few attempts have been made, however, to determine the relation of intelligence to more than one measure of learning,

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children (Kirkwood, 1926); report upon observed material (McGeoch, 1928b); immediate recall of motion pictures (Conrad and Jones, 1929); incidental memory (Willoughby, 1929, 1930); recognition memory (Anastasi, 1930); and learning of college subjects (Drake, 1940; Dysinger and Gregory, 1941; and Carlson, Fisher, and Young, 1945). The important papers by Woodrow (1938, 1940, 1945, 1946) should be read by anyone seriously interested in this problem.

[16] The papers by Hunter (1922), Warden (1924), Husband (1931, 1939, 1941), Thompson and Witryol (1946), and Simrall (1947) are illustrative.

[17] Representative comparisons of the rates of learning of groups at widely separated points on the distribution of intelligence have been made by McGeoch (1925), Hollingworth and Cobb (1928), and Spence and Townsend (1930).

i.e., between intelligence and a pool of two or more sets of learning records, with the result that the addition of more than one measure of learning increases the correlation.[18] Garrett's (1928) zero-order coefficients of correlation between scores on the Thorndike Examination and eight different measures of learning were low, but when a multiple *r* was computed between Thorndike score and the eight measures of learning, the coefficient became 0.53. Corrected for attenuation, it became 0.60. As Garrett points out, this means that although the individual tests have only a little in common with the intelligence test, their own low intercorrelations cause them, when combined, to measure a considerable amount of whatever is measured by the Thorndike test.

Summary and Interpretation

Learning, in its rate and amount, is partially determined by intelligence. This relationship is not impressively high, however, and it would be unwise to assume that ability to learn and intelligence may be identified, or, indeed, that there is a general learning ability. The specific correlations between rate of learning and intelligence which have been obtained probably are too low, however, due to the homogeneity of the samples studied, the unreliability of the scores obtained, and the lack of variety among the learning tasks employed.

INDIVIDUAL DIFFERENCES AND UNCONTROLLED VARIABILITY

Rates of learning are, as we have seen, functions of differences in chronological age, sex, and test intelligence; that is, there are individual differences in rate of learning which are ascribable to these factors. Individuals differ also in personality traits, in emotional condition, in race, and in other characteristics which are to some extent correlated with rate of learning, but the experimental evidence on the influence of these conditions does not yet permit confident generalizations.

[18] Evidence in support of this statement will be found in Haught (1921), Lemmon (1927), Garrison (1928), and Bolton (1931).

When CA, for example, is controlled by selecting subjects who are of the same age, wide differences in rate of learning still appear. The fastest learner in the group may learn from two to ten times as much as the slowest in the same length of time, or may learn a constant amount in a fraction of the number of trials required by the slowest. When the control is more inclusive and the subjects are not only of the same age but also of the same sex, the same class in college, and of equivalent prior training with the material and method used, differences in rate still occur. They may be smaller than when the selection of the subjects is less restrictive, but they are still wide.

Variability occurring among subjects equal in CA, sex, prior training, and sometimes in other respects as well, is usually a residual variability attributable to unknown conditions. Some of these unknown conditions in a particular case may be personality traits and other individual characteristics; some of them may be functions of organic conditions, such as emotional disturbance, those produced by diet or loss of sleep or drugs; some may be a result of the peculiar meanings aroused in the subject by the instructions, the experimenter, or the other conditions of the experiment. Whatever their determining conditions, they comprise an uncontrolled variability.

A question which has long interested psychologists is whether variability, however it is originally produced, is increased, decreased, or unaffected by continued practice. The studies aimed at answering this question involve too many statistical and methodological problems to permit a brief survey to be adequate, and none will be made here.

Fortunately, if the sampling has been carefully chosen and if the external conditions of the experiment are controlled with all the rigidity possible to well-trained experimenters, results can be duplicated and conclusions verified without taking into account all of the suspected sources of uncontrolled variability. Illustrations of the statement that results, even from small samplings, may be duplicated by different experimenters will be found in papers by Barr (1932) and McGeoch (1933). Nevertheless, variability of results represents an important obstacle to the accomplishment of research,

particularly where the demonstration of small differences is important of where it is desired to corroborate quantitative theoretical predictions. The development of selection devices for various types of learning situations which would permit the characteristics of the sample studied to be specified in a quantitative fashion would represent an enormous methodological advance.

REFERENCES

Achilles, E. M. Experimental studies in recall and recognition. *Arch. Psychol., N. Y.*, 1920, 6, No. 44.

Anastasi, A. A group factor in immediate memory. *Arch. Psychol., N. Y.*, 1930, 18, No. 120.

Barr, A. S. A study of the amount of agreement found in the results of four experimenters employing the same experimental technique in a study of the effects of visual and auditory stimulation on learning. *J. educ. Res.*, 1932, 26, 35-45.

Bassett, S. J. Sex differences in history of retention. *Sch. and Soc.*, 1929, 29, 397-398.

Bird, C. *Social psychology*. New York: Appleton-Century, 1940.

Bolton, E. B. The relation of memory to intelligence. *J. exp. Psychol.*, 1931, 14, 37-67.

Boynton, P. L. Are girls superior to boys in visual memory? *J. soc. Psychol.*, 1931, 2, 496-500.

Brown, R. W. The relation between age (chronological and mental) and rate of piano learning. *J. appl. Psychol.*, 1936, 20, 511-516.

Carlson, H. B., Fischer, R. P., and Young, P. T. Improvement in elementary psychology as related to intelligence. *Psychol. Bull.*, 1945, 42, 27-34.

Carmichael, L. A re-evaluation of the concepts of maturation and learning as applied to the early development of behavior. *Psychol. Rev.*, 1936, 43, 450-470.

Carmichael, L. The experimental embryology of mind. *Psychol. Bull.*, 1941, 38, 1-28.

Carr, H. A. *Psychology: a study of mental activity*. New York: Longmans, Green, 1925.

Conrad, H. S., and Jones, H. E. Psychological studies of motion pictures. III. Fidelity of report as a measure of adult intelligence. *Univ. Calif. Publ. Psychol.*, 1929, 3, 245-276.

Conrad, H. S., and Jones, H. E. Psychological studies of motion pictures. V. Adolescent and adult sex differences in immediate and delayed recall. *J. soc. Psychol.*, 1931, 2, 433-457.

Dennis, W. The effect of restricted practice upon the reaching, sitting, and standing of two infants. *J. genet. Psychol.*, 1935, 47, 17-31.

De Weerdt, E. H. A study of the improvability of fifth grade school children in certain mental functions. *J. educ. Psychol.*, 1927, 18, 547-557.

Dietze, A. G. The relation of several factors to factual memory. *J. appl. Psychol.*, 1931, 15, 563-574.

Drake, C. S. The Iota function. *J. educ. Res.*, 1940, 34, 190-198.

Dysinger, D. W., and Gregory, W. S. A preliminary study of some factors

related to student achievement and grades in the beginning course in psychology. *J. gen. Psychol.*, 1941, 24, 195-209.

Emerson, L. L. The effect of bodily orientation upon the young child's memory for position of objects. *Child Develpm.*, 1931, 2, 125-142.

Foster, J. C. Verbal memory in the preschool child. *J. genet. Psychol.*, 1928, 35, 26-44.

Fuller, P. R. Operant conditioning of a vegetative human organism. *Amer. J. Psychol.*, 1949, 62, 587-589.

Gardner, L. P. The learning of low grade aments. *Amer. J. ment. Def.*, 1945, 50, 59-80.

Garrett, H. E. The relation of tests of memory and learning to each other and to general intelligence in a highly selected adult group. *J. educ. Psychol.*, 1928, 19, 601-613.

Garrison, K. C. Correlation between intelligence test scores and success in certain rational organization problems. *J. appl. Psychol.*, 1928, 12, 621-630.

Gesell, A., and Thompson, H. Learning and growth in identical twins: an experimental study by the method of co-twin control. *Genet. Psychol. Monogr.*, 1929, 6, 1-120.

Gilbert, J. C. Memory loss in senescence. *J. abnorm. soc. Psychol.*, 1941, 36, 73-86.

Goldscheider, L. Die Entwicklung des verbalen Gedächtnisses in Alter von 6 bis 18 Jahren. *Beih. Z. angew. Psychol.*, 1932, 64, 9-48.

Gould, M. C., and Perrin, F. A. C. A comparison of the factors involved in the maze learning of human adults and children. *J. exp. Psychol.*, 1916, 1, 122-154.

Gundlach, R., Rothschild, D. A., and Young, P. T. A test and analysis of "set." *J. exp. Psychol.*, 1927, 10, 247-280.

Hallowell, D. K. Mental tests for preschool children. *Psychol. Clin.*, 1928, 16, 235-276.

Haught, B. F. The interrelation of some higher learning processes. *Psychol. Monogr.*, 1921, 30, No. 139.

Heidbreder, E. F. Problem solving in children and adults. *J. genet. Psychol.*, 1928, 35, 522-544.

Hicks, J. A. The acquisition of motor skill in young children: an experimental study of the effects of practice in throwing at a moving target. *Univ. Ia. Stud. Child Welf.*, 1931, 4, No. 5.

Hicks, V. C., and Carr, H. A. Human reactions in a maze. *J. Anim. Behav.*, 1912, 2, 98-125.

Hilgard, J. R. The effect of early and delayed practice on memory and motor performances studied by the method of co-twin control. *Genet. Psychol. Monogr.*, 1933, 14, 493-565.

Hollingworth, H. L. How we learn our "reflexes." *Psychol. Rev.*, 1928, 35, 439-442.

Hollingworth, L. S., and Cobb, M. V. Children clustering at 165 I.Q. and children clustering at 146 I.Q. compared for three years in achievement. *Yearb. nat. Soc. Stud. Educ.*, 1928, 27, Pt. 2, 3-33.

Humpstone, H. J. *Some aspects of the memory span test: a study in associability*. Philadelphia: Psychological Clinic Press, 1917.

Hunter, W. S. Correlation studies with the maze in rats and humans. *Comp. Psychol. Monogr.*, 1922, 1, No. 1, 37-56.

Hurlock, E. B., and Newmark, E. D. The memory span of preschool children. *J. genet. Psychol.*, 1931, 39, 157-171.

Husband, R. W. Comparative behavior on different types of mazes. *J. gen. Psychol.*, 1931, 5, 234-244.

Husband, R. W. Intercorrelations among many learning abilities. *J. genet. Psychol.*, 1939, 55, 353-364.

Husband, R. W. Intercorrelations among learning abilities. IV. Effect of age and spread of intelligence upon relationships. *J. genet. Psychol.*, 1941, 58, 431-434.

Irion, A. L. Reminiscence in pursuit-rotor learning as a function of length of rest and of amount of pre-rest practice. *J. exp. Psychol.*, 1949, 39, 492-499.

Jacobs, J. Experiments on "prehension." *Mind*, 1887, 12, 75-79.

Jersild, A. T., *et al.* Training and growth in the development of children: a study of the relative influence of learning and maturation. *Child Develpm. Monogr.*, 1932, No. 10.

Johnson, B. J. *Child psychology*. Springfield and Baltimore: Thomas, 1932.

Jones, H. E., assisted by H. Conrad and A. Horn. Psychological studies of motion pictures. II. Observations and recall as a function of age. *Univ. Calif. Publ. Psychol.*, 1928, 3, 225-243.

Kantrow, R. W. Studies in infant behavior IV: an investigation of conditioned feeding responses and concomitant adaptive behavior in young infants. *Univ. Ia. Stud. Child Welf.*, 1937, 13, No. 3.

Kirkwood, J. A. The learning process in young children: an experimental study in association. *Univ. Ia. Stud. Child Welf.*, 1926, 3, No. 6.

Knotts, J. R., and Miles, W. R. The maze-learning ability of blind compared with sighted children. *J. genet. Psychol.*, 1929, 36, 21-49.

Köhler, W. *The mentality of apes.* (Trans. from 2nd rev. ed. by E. Winter) New York: Harcourt, Brace, 1925.

Langhorne, M. C. Age and sex differences in the acquisition of one type of skilled movement. *J. exp. Educ.*, 1933, 2, 101-108.

Lemmon, V. W. The relation of reaction time to measures of intelligence, memory, and learning. *Arch. Psychol., N. Y.*, 1927, 15, No. 94.

Lindley, E. H. A study of puzzles with special reference to the psychology of mental adaptation. *Amer. J. Psychol.*, 1897, 8, 431-493.

Marquis, D. P. Can conditioned responses be established in the newborn infant? *J. genet. Psychol.*, 1931, 39, 479-490.

Marquis, D. P. Learning in the neonate: The modification of behavior under three feeding schedules. *J. exp. Psychol.*, 1941, 29, 263-282.

Mateer, F. *Child behavior.* Boston: Badger, 1918.

Matheson, E. A study of problem solving behavior in preschool children. *Child Develpm.*, 1931, 2, 242-262.

Mattson, M. L. The relation between the complexity of the habit to be acquired and the form of the learning curve in young children. *Genet. Psychol. Monogr.*, 193, 13, 299-396.

McGeoch, J. A. The fidelity of report of normal and subnormal children. *Amer. J. Psychol.*, 1925, 36, 434-445.

McGeoch, J. A. The influence of sex and age upon the ability to report. *Amer. J. Psychol.*, 1928a, 40, 458-466.

McGeoch, J. A. Intelligence and the ability to report. *Amer. J. Psychol.*, 1928b, 40, 596-599.

McGeoch, J. A. Changes accompanying practice upon successive samples of verbal material. *J. gen. Psychol.*, 1933, 9, 117-128.

McGinnis, E. The acquisition and interference of motor habits in young children. *Genet. Psychol. Monogr.*, 1929, 6, 203-309.

McPherson, M. W. A survey of experimental studies of learning in individuals who achieve subnormal ratings on standardized psychometric measures. *Amer. J. ment. Def.*, 1948, 52, 232-254.

Miles, W. R. Abilities of older men. *Person. J.*, 1933a, 11, 352-357.

Miles, W. R. Age and human ability. *Psychol. Rev.*, 1933b, 40, 99-123.

Miles, W. R. Training, practice and mental longevity. *Science*, 1935, 81, 79-87.

Mulhall, E. F. Tests of the memories of school children. *J. educ. Psychol.*, 1917, 8, 294-302.

Netschajeff, A. Experimentelle Untersuchungen über die Gedächtnissentwickelung bei Schulkindern. *Z. Psychol.*, 1900, 24, 321-351.

Peterson, J. Learning in children. *In* Murchison, C. (Ed.), *A Handbook of Child Psychology*. (2nd rev. ed.) Worcester: Clark Univ. Press, 1933. Pp. 417-481.

Pilek, E. Die Entwicklung des gegenständlich-anschaulichen Gedächtnisses im Alter von 6 bis 18 Jahren. *Beih. Z. angew. Psychol.*, 1932, 64, 49-106.

Pyle, W. H. *The examination of school children*. New York: Macmillan, 1913.

Pyle, W. H. *Nature and development of learning capacity*. Baltimore: Warwick and York, 1925a.

Pyle, W. H. The relation of sex differences to the kind of material used. *J. educ. Psychol.*, 1925b, 16, 261-264.

Pyle, W. H. *The psychology of learning: an advanced text in educational psychology*. (Rev. and enlarged ed.) Baltimore: Warwick and York, 1928.

Rank, O. *The trauma of birth*. New York: Harcourt Brace, 1929.

Ray, W. S. A preliminary report on a study of fetal conditioning. *Child Develpm.*, 1932, 3, 175-177.

Rethlingshafer, D. Measures of tendency-to-continue. I. Behavior of feebleminded and normal subjects following the interruption of activities. *J. genet. Psychol.*, 1941a, 59, 109-124.

Rethlingshafer, D. Measures of tendency-to-continue. II. Comparison of feebleminded and normal subjects when interrupted under different conditions. *J. genet. Psychol.*, 1941b, 59, 125-138.

Rethlingshafer, D. Comparison of normal and feebleminded children with college adults in their tendency-to-continue interrupted activities. *J. comp. Psychol.*, 1941c, 32, 205-216.

Roberts, K. E. The ability of preschool children to solve problems in which a simple principle of relationship is kept constant. *J. genet. Psychol.*, 1932, 40, 118-133.

Roberts, K. E. Learning in preschool and orphanage children: an experimental study of ability to solve different situations according to the same plan. *Univ. Ia. Stud. Child Welf.*, 1933, 7, No. 3.

Ruch, F. L. Adult learning. *Psychol. Bull.*, 1933, 30, 387-414.

Ruch, F. L. The differentiative effects of age upon human learning. *J. gen. Psychol.*, 1934, 11, 261-285.

Simrall, D. Intelligence and the ability to learn. *J. Psychol.*, 1947, 23, 27-43.

Sorenson, H. Adult ages as a factor in learning. *J. educ. Psychol.*, 1930, 21, 451-459.

Spelt, D. K. The conditioning of the human fetus *in utero*. *J. exp. Psychol.*, 1948, 38, 338-346.

Spence, K. W., and Townsend, S. A comparative study of groups of high and low intelligence in learning a maze. *J. gen. Psychol.*, 1930, 3, 113-129.

Starr, A. S. The diagnostic value of the audito-vocal digit memory span. *Psychol. Clin.*, 1923, 15, 61-84.

Stone, C. P. Maturation and "instinctive" functions. *In* Moss, F. A. (Ed.), *Comparative Psychology*. New York: Prentice-Hall, 1934. Pp. 37-72.

Strayer, L. C. Language and growth: the relative efficacy of early and deferred vocabulary training, studied by the method of co-twin control. *Genet. Psychol. Monogr.*, 1930, 8, 209-317.

Stroud, J. B., and Maul, R. The influence of age upon learning and retention poetry and nonsense of syllables. *J. genet. Psychol.*, 1933, 42, 242-250.

Taylor, C. DeC. A comparative study of visual apprehension in nursery school children and adults. *Child Develpm.*, 1931, 2, 263-271.

Terman, L. M. Psychological approaches to the biography of genius. *Science*, 1940, 92, 293-301.

Terman, L. M., and Oden, M. H. *The gifted child grows up*. Stanford, Calif.: Stanford Univ. Press, 1947.

Thompson, G. G., and Witryol, S. L. The relationship between intelligence and motor learning ability, as measured by a high relief finger maze. *J. Psychol.*, 1946, 22, 237-246.

Thorndike, E. L., *et al.* Adult learning. New York: Macmillan, 1928.

Warden, C. J. The relative economy of various modes of attack in the mastery of a stylus maze. *J. exp. Psychol.*, 1924, 7, 243-275.

Wechsler, D. *The measurement of adult intelligence.* (3rd ed.) Baltimore: Williams and Wilkins Co., Inc., 1944.

Welch, L. The genetic development of the associational structures of abstract thinking. *J. genet. Psychol.*, 1940, 56, 175-206.

Wenger, M. A. Studies in infant behavior III. Pt. I: An investigation of conditioned responses in human infants. *Univ. Ia. Stud. Child Welf.*, 1936, 12, No. 1, 7-90.

Whipple, G. M. *Manual of mental and physical tests*. Baltimore: Warwick and York, 1921.

Wieg, E. L. Bi-lateral transfer in the motor learning of young children and adults. *Child Develpm.*, 1932, 3, 247-268.

Willoughby, R. R. Incidental learning. *J. educ. Psychol.*, 1929, 20, 671-682; 1930, 21, 12-23.

Witty, P. A., and Lehman, H. C. The instinct hypothesis *versus* the maturation hypothesis. *Psychol. Rev.*, 1933, 40, 33-59.

Woodrow, H. The effect of practice on groups of different initial ability. *J. educ. Psychol.*, 1938, 29, 268-278.

Woodrow, H. Interrelations of measures of learning. *J. Psychol.*, 1940, 10, 49-73.

Woodrow, H. Intelligence and improvement in school subjects. *J. educ. Psychol.*, 1945, 36, 155-166.

Woodrow, H. The ability to learn. *Psychol. Rev.*, 1946, 53, 147-158.

XIII

CONCLUDING STATEMENT

INTRODUCTION

The learning process is of fundamental importance in the understanding and control of human behavior. Not only the direction of formal education is concerned. More fundamental determination of the basic modes of behaving and adjusting is also involved. The more stable ways of behaving, which give structure and meaning to the concept of personality, are largely learned, and an understanding of the structure and function of personality necessarily depends upon an understanding of the learning process. There has been entirely too much loose thinking concerning such matters. Few psychologists will deny the importance of experience, particularly early experience, in determining personality variables, yet only rarely have attempts been made to consider these early experiences as a set of conditions for the production of habits, that is, as practice. Too frequently, early experiences are assumed to leave their effects upon personality structure quite independently of any conditions operating at the time of such early experiences. Such thinking is essentially magical in character. It should be taken as a fundamental postulate that the experiences and reactions of the individual will leave effects upon future behavior only insofar as they have occurred under proper conditions of practice.

The conditions of practice, as we have seen, are extremely numerous and extremely complex. Any attempt to state learning in terms of a single variable or a limited number of conditions necessarily represents a simplification of the actual state of affairs. Such a simplification is justifiable on either theoretical or practical grounds if it results in the consolidation of specific conditions into a smaller number of meaningful classes of conditions. Any such classificatory scheme inevitably does violence to some of the facts, but under-

standing this, the conclusions which are outlined below seem to be in order.

THE CONDITIONS OF LEARNING

From the standpoint of controlling the learning process, we are interested primarily in the responses made by the learner. In view of this, it is most practical to consider learning in terms of relationships between stimuli and responses. We usually wish to promote the occurrence of particular responses under specific circumstances. Thus, when the teacher asks the pupil the question, "When did Columbus discover America?" we wish to hear the response, "1492." This response, it should be noted, must occur only under a limited set of stimulating circumstances. It should not be given when the question concerns the signing of the Magna Carta, the death of Alexander the Great, or the outbreak of the French revolution. In the same way, we are concerned that the piano student strike the correct sequence and combination of keys in response to the line of musical notation—and no other. What internal processes intervene between the reception of stimuli and the occurrence of responses makes no difference to us at this level of description, although such knowledge would be desirable from the standpoint of increasing neurophysiological knowledge. Psychologically, however, we may be interested entirely in the prediction of responses under particular circumstances. We may use intervening variables, as Hull does, to aid in this prediction, but these need have no independent reality. Two different systems for predicting from stimulus to response should be evaluated entirely in terms of predictive adequacy and internal consistency. A system, inferior in predictive power, should not be preferred because it represents a "sounder" physiological approach. The reason for this is apparent. A perfectly sound physiological system will, of necessity, have perfect predictive powers.

We may also note that two systems which predict the same response phenomena throughout have the same mathematical properties, regardless of the verbalizations which surround them. For our purposes, then, learning may be considered as existing between stimuli and responses and consists in a change in the probability

(or other measure) that a particular response will occur under a particular set of circumstances.

A number of conditions operate to bring about the changes we call learned. One of the most apparent is that the stimulus and the response to become connected must occur together in time. This is the law of contiguity, the operation of which is so well known as to make examples and explanation unnecessary. It could be noted, perhaps, that the relationship of strict contiguity (simultaneous presentation of stimulus and occurrence of response) is probably not the best one for the best one for the establishment of habits. Stimulation should slightly precede response for the learning to be most efficient. It may also be noted that the mediation of language can operate in man to reduce the need for close temporal contiguity. Thus, we can learn to respond to the symbolic representation of a situation and later transfer that learning to the situation itself. The success of such training probably depends upon the degree to which the appropriate symbolic-motor habits have been established in the past.

Temporal contiguity, alone, however, is not productive of learning. Reinforcement or reward is also necessary.[1] This reinforcement, in order to be effective, must closely follow the response which is to be learned. We thus have two laws of contiguity, the one between stimulus and response and the other between the response and the reward. The concept of reinforcement, in turn, implies the condition of motivation. This motivation must operate either at the time of learning or must have operated at some prior time, in order that the reinforcement will be effective. The reason for this is that a reward consists either of motive reduction at the time in question or of the administration of a secondary reinforcing stimulus. The secondary reinforcing stimulus, however, gains its power only through the

[1] Whether reward is, itself, a fundamental condition or whether it may be deduced from the law of contiguity after the manner of Guthrie, need not concern us here. Although it is doubtful that an operational distinction can be made between the reintorcement and the Guthrian positions in this matter (cf. Chap. II), it seems best to include reinforcement as one of the fundamental conditions. In the practical control of learning, at any rate, it is of very great importance.

mechanism of previous association with motive reduction. Motivation, of course, plays other roles in the determination of learning and performance than the one mentioned here.

Under such conditions of stimulus, response, and reward, learning will occur. Basically, however, learning appears to be a slow process requiring, even under excellent conditions of practice, a great many repetitions. This need for frequency, however, can be reduced if the individual's past learning can transfer positively to the new practice situation. Under such circumstances, acquisition of a specific habit may be quite rapid. A complete account of such learning, however, would have to include a statement of the acquisition of those earlier habits which have this facilitating influence.

One phase of the psychology of learning which has been somewhat neglected experimentally, but which is of great significance in the everyday learning of people, is the "discovery" phase. In almost every learning situation, a certain amount of so-called trial-and-error behavior precedes the making of the correct response. In most experimental situations for the study of learning, and, for that matter, in many of the formal educational situations provided by the school, this discovery phase is minimal in scope. On the other hand, in many of the informal learning situations which abound in everyday life, the discovery phase is often the most prominent feature of the learning process. Discovering correct—that is, rewarding—responses under such circumstances is, in turn, dependent upon a number of other conditions. Basically, of course, discovery depends upon the variability of behavior. The more variable the behavior, the more likely it is that an adequate response will be discovered. The variability of behavior depends upon the extinctionability of the inadequate responses, upon the variability of the stimulating circumstances, and upon the persistence of behavior. In the discovery process, motivation plays a number of roles. A certain strength of motivation is necessary to keep activity at a relatively high level. On the other hand, very high levels of motivation tend to restrict behavioral variability because they impede the process of extinction. At the same time, motivational stimuli tend to give direction to the ongoing behavior by means of transfer of training.

Thus, the hungry animal tends to respond in ways which have led to food on previous occasions. In the same way, the mental patient suffering from anxiety tends to repeat those symptomatic responses which have been fear-reducing in the past.

SOME COMPLEXITIES OF THE LEARNING PROCESS

It should also be recognized that the learner is usually acquiring a number of habits simultaneously. These habits may involve such distinct systems of response that their acquisition proceeds relatively independently. More often, perhaps, there is an interaction among such habits, each being modified in various ways by the other. By this means, the child in school may not only acquire certain skills in, let us say, geography, but he may simultaneously learn habits of studying and emotional reactions to the school situation which will facilitate and/or inhibit the present course of learning of geography and which will influence further school achievement in various ways. Indeed, all learning, beyond the very earliest period of infancy, occurs against a background of previously established habits, although it is often difficult to identify the particular bits of prior practice which are currently having effect.

Simultaneously with the learning of new habits, decremental factors are operating to modify both the new habits and also those which have been previously established. These decremental forces are probably of two types, those based on a mechanism similar to work decrement and those resulting from interference between competing habit systems. When we consider the acquisition of a particular skill, we should note that decremental factors operate in various ways. It would be a mistake to consider that these decremental factors always have a deleterious effect upon the habit involved. In the acquisition of any habit, incorrect as well as correct responses are apt to be acquired, and the loss of these is necessary if the final performance is to be entirely successful. While the dropping-out of incorrect responses may be most obvious in the case of learning serial acts (lists of words, maze habits, etc.), it plays an equally important role in the learning of any motor skill. By means of the operation of decremental factors, motor performances

tend to become increasingly efficient and better-coordinated. Superfluous responses and awkward movements which tend to interfere with the performance of the skill tend to drop out, either as a result of the extra effort which their performance involves (the skilled performance is usually far less effortful than the unskilled one) or because of successful competition from correct or more adequate responses. This process is rarely complete, however, and the occurrence of superfluous and interfering movements is one of the limiting factors in the acquisition of any skill.

Each new learning is influenced by older habits, but the influence of such transfer operates in both directions, that is to say, new learning has its effect upon the performance of previously established habits. This influence may be facilitative or it may disrupt or modify the older reaction patterns. The process of development contains numerous examples of reaction patterns which become established and then are displaced by the subsequent learning of other ways of reacting in that situation. Similarly, forgetting is determined, at least in large part, by the learning of new and different habits between the time of original learning and later recall.

In addition to these factors, which influence the learning process, it must be remembered that individual differences play an important role in the acquisition process. These individual differences in the learning abilities (and it should be remembered that these are more likely to be plural than singular) cause variations in the rate of learning and also in the level of final achievement regardless of the amount of practice. Furthermore, the influence of individual variation in learning abilities undoubtedly becomes magnified as time goes on owing to the factor of transfer of training. In this way the able child, who learns much early in life, faces a new learning situation armed with both superior ability and a greater wealth of retained experience which can be brought to bear on the new problem.

THE INFLUENCE OF LEARNED DRIVES AND REWARDS

One of the earliest products of learning in the child is the formation of secondary motives and secondary rewards. His first learned responses, of course, are necessarily based on biological motives. Secondary drives and rewards are not long delayed in their appearance, however. Stimuli associated with the primary drive gratifications of feeding and comforting early take on secondary reinforcing characteristics. For example, since the mother or nurse is typically a constantly recurring stimulus in the situation wherein primary drives are reduced, the presence of the mother, the sound of her voice, and other stimuli associated with her presence usually become powerful secondary rewarding stimuli for the child. In a like manner, stimulation of the oral regions becomes increasingly satisfactory to the child because of the intimate association with feeding.[2] Many of the learned responses of children are based upon such secondary reinforcing states in the sense that children tend to learn responses which are followed by such rewarding stimuli as the sight of the mother, her vocal responses, etc. By this means are learned many of the "attention-getting" responses of early childhood, and it is apparent that thumb-sucking may be reinforced by this mechanism.

Secondary motives are also learned early in life, and their reduction is an important source of reinforcement for further learning. As we have seen, the best known, and probably the most important, secondary motive is fear. Fears are acquired through the association of neutral stimuli with the response to painful stimulation. Such learning occurs both as a result of the normal painful events of childhood and also as a result of systematic inculcation of fear, as by parental punishment. Responses based on learned fears are acquired because they tend to remove the fear-eliciting stimuli and hence to reduce the fear itself. This is a relatively easy type of

[2] It is likely that the mouth, together with the anal and genital regions, is an innately "erogenous" zone. By this is meant that stimulation of these areas is likely to be reinforcing regardless of past training. An excellent case can be made, however, for the position that each of these regions acquires "erogenous" properties through the mechanism of secondary reinforcement.

learning, so long as the fear-eliciting stimuli are external to the learner and he has no particularly strong habitual tendencies to approach these stimuli. Thus, children typically experience no difficulty in learning to avoid hot stoves and open electric light sockets. When, however, a child is trained in such a way as to become afraid of his own motives (which are, of course, within himself), the learning problem becomes much more difficult to solve and conflict reactions, oftentimes of a severe nature, can result. It is this latter factor which makes psychologists wary of advising the use of punishment as a pedagogical technique to parents and teachers. The difficulty is not that punishment does not produce learning. Rather, it is that the immediate and delayed learning resulting from punishment may be far greater in scope than is intended. The immediate learning of fear as well as avoidance of the punished responses is almost certain to result. This fear, under certain circumstances, can set for the individual a new learning problem based upon this secondary motivational state, and the solution of this learning problem may involve the acquisition of fear-reducing responses which are symptomatic in character.

THE DESIRABILITY OF HABITS

In this connection, it is apparent that not all learning need result in habits which are desirable from the standpoint of society, or indeed, from the standpoint of the learner himself, when the matter is logically considered. Most learning, it is true, does result in socially desirable behavior because society arranges the conditions of reinforcement to promote this end. There are, however, numerous exceptions to this rule. In such cases it is always necessary to look for the sources of reinforcement which are responsible for the learning of the habits in question and which serve to maintain these habits once they are established. Whenever we observe an individual engaging in persistent folly which brings him (to all appearances) only grief and disappointment, we must be prepared to find that this folly is being regularly reinforced. Sometimes, the source of reinforcement is fairly apparent, as when the individual lives under rather special environmental conditions which give him that rein-

forcement. More frequently, however, the source of reinforcement is internal to the learner as, for example, in the case of reinforcement by fear reduction. Furthermore, even when such reinforcing states are found and identified, it is rarely true that the situation can be easily changed. The neurotic individual who controls (reduces and avoids) his secondary drive of fear by engaging in compulsive acts receives an overwhelming number of reinforcements of this behavior in the course of a relatively short time. Such reinforcements result in a pattern of behavior not easily to be changed. Similarly, the student who is consistently rewarded for indolence by his associates is not likely to be made more industrious by infrequent exhortations from the dean to study more diligently. In so many cases, the rewards for undesirable behavior are immediate and frequent, while the rewards for more acceptable behavior are infrequent and remote that little can be done to change the established behavior patterns.

This tendency to learn the "wrong" things is not confined to the learning of symptomatic responses. Many a "Sunday golfer" learns inefficient golfing habits because they are followed by occasional or partial success. In a similar fashion, many musical talents are lost to the world because, as a beginner, the student acquires incorrect habits which make for greater temporary reinforcement. Thus, the budding violinist acquires a violent vibrato early in his career, partly because it masks the defects of intonation which otherwise mar his playing. Later, however, this habit will stand as a barrier to acquiring technical skill. In a similar fashion, the academic student may acquire habits of flashy, but superficial scholarship which bring him immediate rewards at an elementary level, but which may prevent his success in the more rigorous work of professional or graduate college.

SOME METHODOLOGICAL PROBLEMS

In the few pages which remain, it would be presumptuous to attempt to evaluate the current status of research in the field of human learning from the standpoint of its methodological adequacy. This is particularly true, in view of the fact that much meticulous

work has been done on this subject. The reviews of methodology by Melton and by Hilgard, to which we have already referred, are, perhaps, the most outstanding examples of such work. Nevertheless, a few words should be said on this subject even though they are only introductory in character. In general, experimental situations for the study of human learning have tended to be too specialized and not representative enough of the variables which determine the learning process during the ordinary psychological development of individuals. To recognize this is not to advocate, as many do, that learning should be studied only in "real life" situations. To follow such a course would cause a regression of our scientific progress back nearly to the anecdotal level. Instead, more of the variables which enter into the learning of special skills, academic habits, and modes of adjustment of everyday life should be incorporated into standardized experimental situations for purposes of laboratory investigation. In some cases, no doubt, existing situations can be modified for this purpose. In other instances it is probable that new experimental situations will have to be developed. Promising beginnings have been made along such lines. In the general field of motor-skills research, a great variety of new experimental techniques and situations have been and are being constructed. Similarly, learning as a function of certain personality variables is being investigated under modifications of existing conditioning and verbal learning techniques. Other examples could also be cited. It is probable that the study of learning will develop in such directions during the next few years.

If the situations for the study of learning have been somewhat restricted in scope, they certainly have not suffered from lack of variability of the specific conditions operating in each of them. Almost all possible variations of these conditions have been employed, but, more often than not, this variation has been gratuitous. Thus, two studies of rote learning may employ two different lists of nonsense syllables, two different rates of presentation, two different degrees of inter-trial distribution of practice, and so on. As a result of this gratuitous variation of conditions, it is almost impossible to compare results from different experiments or to draw

general conclusions from a number of seemingly interrelated experiments. The situation resembles that which would exist in physics if each physicist used the width of his thumb as a measure of length, the weight of his left shoe as a unit of mass, and the time required to say "abracadabra" as a standard of time. This problem of the lack of standardization of experimental conditions has been recognized for a long time. Very little has been done about improving the situation, however, and it would be exceedingly optimistic to assume that the problem will be solved in the immediate future. Not only are objections voiced to standardization on the (false) grounds that experimental work would be restricted, but it must also be recognized that construction of standard conditions for even a single type of task would require many months or years of patient and rather pedestrian research work.

Nevertheless, standardization should be undertaken for a number of the more commonly used research situations. Let us examine one such situation—the rote-learning situation is an example—and see what is involved in such standardization. In the rote-learning situation, a first requirement is the calibration of a considerable number of lists of materials. We should have several lists of equivalent difficulty in each of several lengths and levels of difficulty. Perhaps this should be done with several types of materials such as four-letter nouns, two-syllable adjectives, nonsense syllables, and consonant syllables. It should be noted that such calibration will have to be accomplished for whole lists rather than for the individual component items. This is because it is known that there is a powerful interaction between item difficulty and difficulty of the list in which the item is embedded. In the same way, it would be necessary to construct lists bearing calibrated degrees of similarity to each other for use in studies of transfer of training. Once calibration of such lists has been accomplished, standardization of other aspects of the experimental situation would not be difficult to accomplish. All that is needed, here, is agreement to use, let us say, a two-second rate of presentation, four seconds between trials, a particular criterion for learning, and so on.

In addition to this, it would be a great help to have some method

of specifying the ability of the subjects. Many discrepancies between experimental results undoubtedly arise from differences in the abilities of the groups tested. An aptitude test, then, for the rote-learning situation would be of considerable value. If we possessed such an instrument we could standardize samples as well as situations. Just as the experimental study of vision depends upon the concept of the "standard observer," so the study of rote-learning could depend upon the concept of the "standard sample." Some such device will probably have to be developed before precise, quantitative relationships can be formulated.

To accomplish such a program as the one outlined above will require a very extensive amount of experimental labor of a rather unexciting nature. In some learning situations it is probable that standardization will be easier to accomplish than in the example we have described. Regardless of the amount of labor involved, however, the need for such standardization is clear. A beginning could be made fairly painlessly if agreement could be reached on a few matters such as rate of presentation, degree of massing of trials, the degree of perfection which will be employed as a performance criterion, and so on. This would insure at least a rough comparability of results obtained in different experiments, even though exact calibration of those situations didn't exist. Unfortunately, there is little evidence that research workers in the fields of learning are concerned with this problem to a sufficient extent to come to agreement in even such preliminary ways.

Despite the gloomy outlook we have just described, however, there is every reason to believe that standardization will be achieved eventually if not immediately. Within some limited experimental situations a fair degree of standardization has already been achieved. Series of experiments by single research workers have often been conducted under comparable conditions from one experiment to the next. Furthermore, the increasing emphasis upon quantification can lead only in the direction of standardization. Until recently, and, indeed, up to the present time, the great bulk of learning research has been of the "more or less" variety. That is, the major objective has been to obtain a "significant" difference as a result

of two (or more) treatments. The reason for this has been that much of the work has been exploratory in character, an attempt to isolate the important variables of the learning process rather than to determine the relationships between them. Vague and unprecise theoretical formulations have also been responsible. In a great many cases, the most precise deduction which can be drawn from a theory is that "A should yield better learning than B." This evidently unsatisfactory state of affairs is now being improved. Current theory increasingly permits the deduction of empirical phenomena in a precise, quantitative fashion. There seems little doubt that this tendency will continue and that eventually a precise, quantitative science of human behavior will be developed.

AUTHOR INDEX

SUBJECT INDEX

Other Longmans Books

PROJECTIVE TECHNIQUES
By JOHN ELDERKIN BELL. 1948.

STATISTICS IN PSYCHOLOGY AND EDUCATION
By HENRY E. GARRETT.
3rd edition. 1947.

PSYCHOLOGICAL TESTING
By JAMES L. MURSELL.
2nd edition. 1949.

PSYCHOLOGY OF ABNORMAL PEOPLE
By J. J. B. MORGAN *and* GEORGE D. LOVELL. 1948.

PSYCHOLOGY AND ART OF THE BLIND
By GEORGE REVESZ. 1950.